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MODERN ASTRONOMY

Its Rise and Progress

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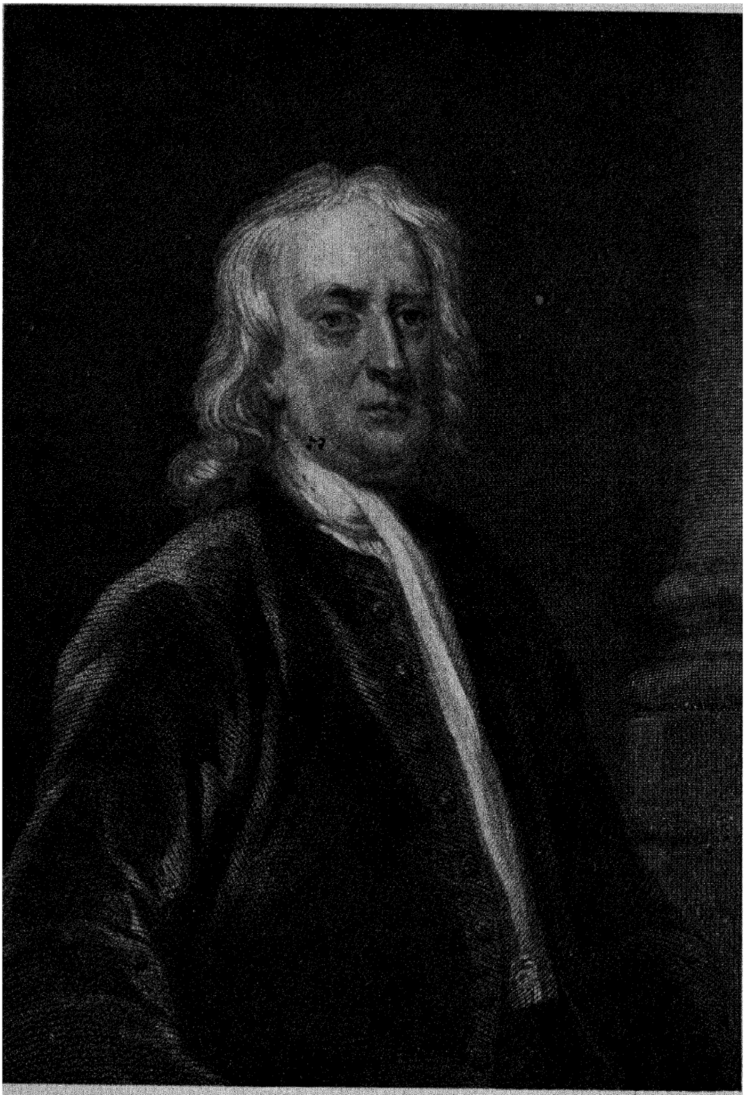

ISAAC NEWTON

MODERN ASTRONOMY

Its Rise and Progress

By
HECTOR MACPHERSON
M.A., Ph.D., F.R.S.E., F.R.A.S.
Author of *Herschel*
The Romance of Modern Astronomy
A Century's Progress in Astronomy
&c.

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1926

TO

MY WIFE

PREFACE

THE Senatus of Aberdeen United Free Church College did me the honour of appointing me to the Thomson Lectureship in Natural Science for the session 1925–6. For permission to publish these lectures in book form, I have to record my sincere thanks to the Senatus. Some of the material had already appeared in articles contributed by me to *The Observatory* and to *Discovery*, and I am under obligation to the Editors of *The Observatory* and to the Trustees of *Discovery* and Mr. John Murray, the former publisher of that journal, for their permission to use that material in these lectures.

I have also to record my indebtedness to Professor A. S. Eddington, LL.D., F.R.S., for his kindness in reading over the two concluding lectures, dealing with the cosmological and cosmogonic questions on which he is one of the chief modern authorities. My thanks are likewise due to Dr. Harlow Shapley, director of Harvard College Observatory. U.S.A., and to Dr. V. M. Slipher, director of the Lowell Observatory, Flagstaff, Arizona, U.S.A., for communicating to me their latest conclusions on the spiral nebulae and the giant planets respectively.

EDINBURGH,
June 1926.

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I

INTRODUCTORY

THE modern science of astronomy is a child of the Renaissance. The day of its nativity was the 23rd of May 1543, when Copernicus received on his death-bed an advance copy of his *magnum opus*—the volume which was to revolutionize human thought—*De Revolutionibus Orbium Coelestium.* There had been, of course, Copernicans before Copernicus. Two or three ancient Greek thinkers—cranks and faddists so-called—had at least toyed with the idea of a rotating and revolving Earth ; even at the close of the Middle Ages, Nicholas of Cusa discussed the probability of the motion of our world, but this was in the nature of pure speculation, and attracted practically no attention. Copernicus, on the other hand, marshalled fact after fact to demonstrate the truth of his contention. Little wonder that within a comparatively short time the orthodox party—scientists and theologians alike—had *De Revolutionibus* placed on the index of prohibited books.

But, as the ancient proverb has it, great is the truth and it shall prevail. And the heliocentric theory of the Solar System was no exception to the rule. The earliest period in the history of modern astronomy was the period which lasted from 1543 to 1687, during which the Copernican system, from being a heresy, became an accepted scientific theory. This was a period of bold and brilliant astronomers, whose names are household words. The harvest was plenteous, and the labourers were few, and so discovery after discovery was made, and each discovery strengthened the system of Copernicus. Tycho Brahe, the great Danish observer, did not himself accept the new system; but the observations which he made from his island observatory at Uraniaborg, in the Sound, effectively shattered the old, for these observations formed the raw material on which the genius of Kepler was brought to bear, and from which he extracted the three laws of planetary motion which still further simplified the Copernican system. These laws, which Kepler established for Mars, and correctly surmised to be valid for the other planets, are as follows: (1) The planets describe ellipses, the Sun being in one of the foci. (2) The straight lines joining the planets to the Sun sweep out equal areas in equal times. (3) The squares of the times of revolution of the planets round the Sun are proportional to the cubes of their mean distances from the Sun. These laws made the Solar System intelligible, and showed it to be a system in actuality. Copernicus had not divested himself of the ancient belief that the only possible orbit was a circle, and so, in order to explain many of the

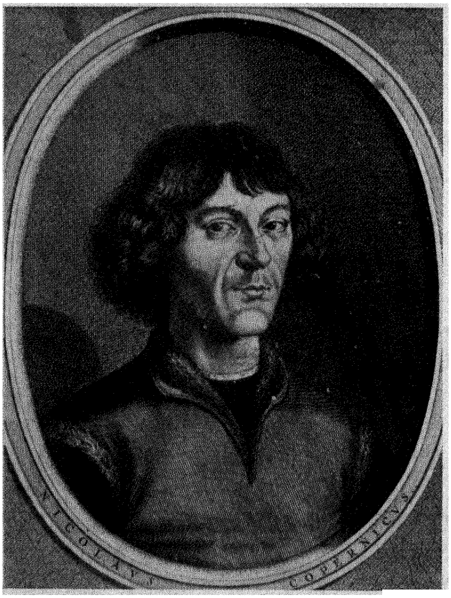

FIG. 1. NICOLAS COPERNICUS

apparent irregularities of the planetary motions, he had been obliged to retain a number of epicycles even on his heliocentric orbits. Kepler's laws swept away these epicycles, thus proving that Nature works by the simplest methods.

The work of Kepler's older contemporary, Galileo, was still more potent in shattering the old system and establishing the new on a firm basis. His researches on falling bodies showed that all bodies fall from the same height in equal times—a law the truth of which he proved in dramatic fashion in the teeth of furious opposition on the famous occasion when he dropped a ten-pound weight and a one-pound weight from the top of the leaning tower of Pisa; also, he detected the relation between the space covered by a body in its descent and the time required for that descent.

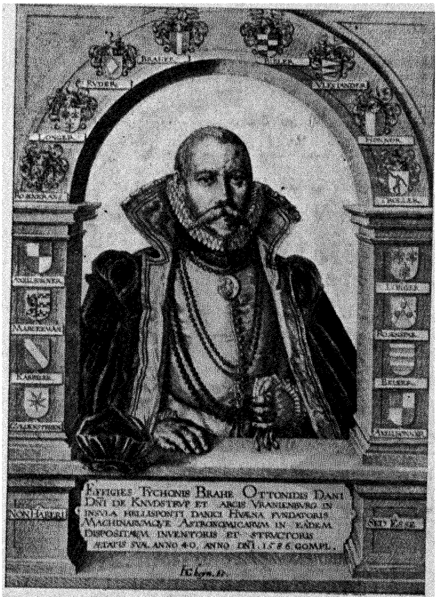
EFFIGIES TYCHONIS BRAHE OTTONIDIS DANI
DNI DE KNVDSTRVP ET ARCIS VRANIENBVRG IN
INSVLA HELLISPONTI DANICI HVÆNA FVNDATORIS
MACHINARVMQVE ASTRONOMICARVM IN EADEM
DISPOSITARVM INVENTORIS ET STRVCTORIS
ÆTATIS SVÆ ANNO 40. ANNO DNI. 1586 COMPL.
NON HABERI
SED ESSE

FIG. 2. TYCHO BRAHE

These discoveries prepared the way for the work of Newton on gravity. As Lagrange said long afterwards, philosophers before Galileo ‘considered the forces which act on bodies in a state of equilibrium only, and although they could only attribute in a vague way the acceleration of heavy bodies, and the curvilinear movement of projectiles, to the constant action of gravity, nobody had yet succeeded in determining the laws of these daily phenomena on the basis of a cause so simple. Galileo made the first important steps.’ The three laws of motion enumerated by Newton in 1687 were in the main based on Galileo’s work. These laws are that (1) every body continues in its state of rest or of uniform motion in a straight line, except in so far as it may be compelled by force applied to it to change that state; (2) change of motion is proportional to the applied force, and takes place in the direction in which the force acts; and (3) that to every action there is always an equal and contrary reaction, or the mutual actions of any two bodies are always equal and oppositely directed. The first two of these laws were implicit in Galileo’s work.

Galileo’s popular fame rests, however, on his work as the virtual inventor of the telescope. He did not make the first telescope, but he first directed the telescope to the sky. As his admirer, Castelli, said not long before his death, Galileo’s eye ‘may be truly said to have seen more than the eyes of all that are gone and to have opened the eyes of all that are to come’. His pioneer telescopic work resulted in his discoveries of the mountains and plains on the Moon, of sunspots, of the phases of Venus and of Mars, of the satellites of Jupiter and of the stellar nature of the Milky Way. In this way he added quite considerably to the sum-total of men’s knowledge of the Universe; while each of these discoveries, more especially those of the phases of Venus and the moons of Jupiter, still further strengthened the Copernican system by weakening the authority of Aristotle and of Ptolemy.

The first period in modern astronomy was brought to a close by the work of Newton. Building on the dynamics of Galileo and the planetary laws of Kepler, Newton gave by his theory of gravitation the explanation of these empirical laws. Grant that ‘every particle of matter in the Universe attracts every other particle with a force varying inversely as the square of their mutual distances, and directly as the mass of the attracting particle’, and from this there follow the orbital revolutions of the Moon and planets, and such familiar everyday phenomena as the motions of falling bodies, so that a large number of isolated facts, hitherto regarded as quite disconnected, were shown to be intimately related as the consequences of universal law. Whatever be the ultimate nature of gravitation, whether it be a force, as Newton,

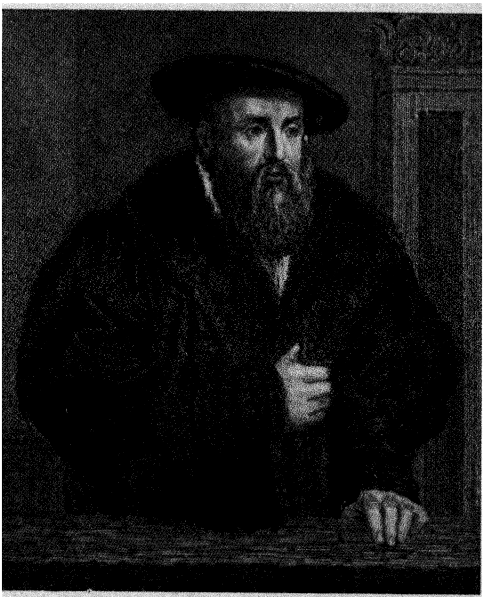

Fig. 3. JOHANN KEPLER

under the influence of the thought-forms of the time, regarded it, or a property of the 'space-time continuum', as Einstein holds, the supreme achievement of Newton was that under the sweep of his law he effected a synthesis of the disconnected facts of astronomy and dynamics, and that the motions deduced from his law agreed almost exactly with the motions observed on the Earth and in the sky.

The second period in modern astronomy opened with the appearance of the *Principia* in 1687, and may be said to have closed exactly a century later with a notable paper addressed to the French Academy, in which Laplace set forth his explanation of the cause of the acceleration of the Moon's mean motion. During this period, theoretical and practical or observational astronomy developed side by side. Curiously enough, the work of Newton was carried on, not by his own countrymen, but by a school of French mathematicians, chief among whom were Euler, Clairaut, D'Alembert, Lagrange, and Laplace. The main aim of this school was to show that the Newtonian theory was capable of explaining the observed motions of the bodies in the Solar System. The problem before these mathematicians was: given the eighteen known bodies in the Solar System—Sun, planets, and satellites—and their positions and motions at any given time, to deduce from their influence on one another in accordance with Newton's law their positions and motions at some later period, and to prove that these are in agreement with actual observation. The problem was a formidable one, and its complete solution is not yet, but the French mathematicians attacked it bit by bit, solving here a particular case and there a particular case, until by the end of the eighteenth century the last of the outstanding anomalies had been removed. Laplace's *Mécanique céleste* was a summation of the triumph of the great French mathematicians. Of this great book it has been said:[1]

> 'The work is a record of unmixed triumphs. . . . With grave exultation Laplace proceeds from point to point, recounting the

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[1] By the late Miss A. M. Clerke, the astronomical historian.

events of the campaign, commemorating the battles won by the brilliant staff of mathematical heroes to which he himself belonged. . . . He scarcely looked beyond. There was indeed at that time no " beyond ", where his methods of investigation were applicable. The *Mécanique céleste* hints at no unsatisfied ambitions.'

It is evident that during the period the observational side of astronomy was subsidiary to the theoretical. Observation was, however, absolutely necessary to the mathematicians; theory had to be checked by observation at every stage. For some time after the invention of the telescope, the chief work of astronomers was observational, in the field of what is commonly called descriptive astronomy. Galileo had been the pioneer in this field, and several others followed in his steps during the earlier half of the seventeenth century. Thus

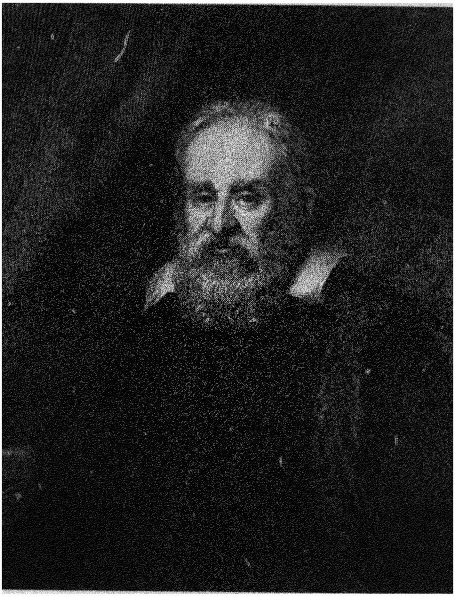

FIG. 4. GALILEO GALILEI

Hevelius in Germany with the aid of his long cumbersome telescopes made the first map of the Moon, while in Holland Huyghens discovered Saturn's ring and largest satellite. Further, Cassini, the Italian astronomer who became the first director of the Paris Observatory, detected the more obvious facts about the planets—their rotation periods, chief surface-markings, and their more prominent moons, while Roemer's observations of Jupiter's satellites led him to the discovery of the velocity of light.

But a limit was soon put to work of this kind by the limitation of telescopic power. The possibilities of the refracting telescope were soon to be temporarily exhausted by the unpleasant effect known as ' chromatic aberration ', an effect due to the dispersion of white light into its component colours on passing through lenses. Efforts were made to counteract this evil by such men as Huyghens and Hevelius, who constructed telescopes of enormous focal length; but the performance of these was not satisfactory. Not till 1758 did Dollond, an English optician, describe in a paper to the Royal Society a device whereby chromatic aberration could be eliminated. Meanwhile, Newton had about 1665 constructed the first reflecting telescope, the mirror of which was one inch in diameter; but for many years this instrument did not advance beyond its infancy.

Descriptive astronomy then came to a standstill, and during the later seventeenth century and most of the eighteenth observational astronomy was virtually astronomy of position. The pressing problem before astronomers was the accurate determination of the positions of the celestial bodies. This required not high magnification of the images of the heavenly bodies but refined methods of measurement. A young English mathematician, William Gascoigne, whose promising career was cut short at the battle of Marston Moor, found that the telescope could be used not only for observing the physical characteristics of the heavenly bodies, but as an instrument of precision, by means of which angular distances and positions could be measured. In fundamental astronomy the chief desideratum was not size of telescope, but accuracy and reliability of methods of measurement, combined with skill on the part of the observer.

To the work of fundamental astronomy—the field in which Tycho Brahe had been the last great pre-telescopic worker—Greenwich Observatory was devoted from its foundation in 1675. This great institution originated in a beneficent caprice on the part of a monarch to whom posterity owes but little, Charles II, who declared that he must have the places of the stars re-observed, so that his seamen might have the advantage of improved knowledge of longitudes. To the new post of Astronomer Royal there was appointed John Flamsteed, who was already known as a practical astronomer. Under Flamsteed and his successors Halley and Bradley, who held the post between them until 1762, the chief work of the Observatory consisted in securing more and more accurate observations of the positions of Moon, planets, and stars. Indeed, the work of Bradley, in particular, was the last word in refinement, and is now regarded as the foundation of modern statistical astronomy. Thus, while Newton's direct successors in gravitational astronomy were the French mathematicians, the English observers carried through the spade-work necessary for the development of the Newtonian theory. Thus, as an acute historian has pointed out, ' practical and theoretical astronomy advanced in parallel lines in England and France respectively, the improvement of their several tools—the telescope and the quadrant on the one side, and the calculus on the other—keeping pace. The whole future of the science seemed to be theirs. The cessation of interest through a too speedy attainment of the perfection towards which each spurred the other appeared to be the only danger it held in store for them.' In fact the imposing edifice of astronomical knowledge seemed to be nearing its completion.

But just as the second or gravitational period of astronomy was closing, a new chapter in astronomical history was opened by the emergence of the elder Herschel as an astronomer of the first rank. This third period, which is not yet concluded, may be called pre-eminently the era of descriptive astronomy. Gravitational and practical astronomy have certainly gone forward with steady progression, but their rate of progress has been slow compared to the amazing strides made by descriptive astronomy. The modern period in astronomy may be said to date from the 13th of March 1781. On that evening, with his home-made telescope, an unknown German observer, by profession a music-teacher in the old English town of Bath, jotted down in his journal the following note, in somewhat doubtful English: 'In the quartile near Zeta Tauri, the lowest of two is a curious either nebulous star or perhaps a comet.' This 'comet' proved to be no comet at all, but an exterior planet. And so unknown planet and unknown astronomer swam into the ken of humanity.

The Latin inscription on the tomb of Sir William Herschel in the church of Upton, near Slough, claims that 'he broke through the barriers of the skies'. There can be no doubt of the justice of this eulogy. Herschel certainly, as he himself claimed in old age, 'looked further into space than ever human being did before him'; he opened to mankind the vista of virtual limitlessness in space and stretched out further and further the duration of time. This was perhaps Herschel's most striking achievement; but it was by no means his only one. The whole field of stellar astronomy was enriched by his investigations. The motion of the Sun through space, which proved beyond a doubt the essential kinship of Sun and stars; the existence of revolving double stars, moving round a common centre of gravity in accordance with the Newtonian law; the existence of multitudes of faint filmy objects called nebulae—these were three of the most striking of Herschel's discoveries in the realm of stellar astronomy. But the whole field of descriptive astronomy was indeed his province, and in the development of this branch his work marked an epoch. In the last analysis, however, his success was due not only to his own genius for observation, but to what he liked to call the 'space-penetrating power' of his telescopes. Not the least important service which he rendered to science was his remarkable development of the powers of the telescope—all the more remarkable in view of the fact that he served no apprenticeship as an optician. In this department, as, indeed, in all else, he was self-made; he owed nothing either to colleges or to workshops.

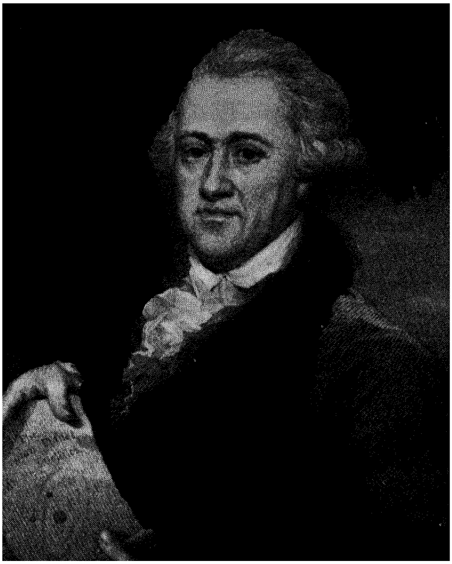

Fig. 5. WILLIAM HERSCHEL

Born at Hanover in 1738, Friedrich Wilhelm Herschel was the son of Isaac Herschel, bandmaster in the Hanoverian Guard. At the Garrison School at Hanover Herschel easily took first place among his fellow scholars, but the straitened condition of the family left him no alternative but to enter the Guards as a musician. His military career was not of long duration. After the disastrous battle of Hastenbeck in 1757—in which engagement the Anglo-German forces were routed by the French—young Herschel, in his own quaint words penned long afterwards, 'resigned' from the service. Whether or not this 'resignation' was legally 'desertion' is a technical point which need not detain us. But, finding that he was 'in danger of being pressed for a soldier' after he left the service, Herschel fled to Hamburg and thence by sea to England, where he had for several years a severe struggle for a livelihood. At length, however, fortune smiled on him, or rather his indomitable optimism and perseverance carried him through, and in 1767 he was installed as organist of the Octagon Chapel at Bath. In addition to this appointment he was director of concerts and a teacher of music, so that his life at this stage was a very crowded one.

A boyish interest in astronomy was revived by the perusal of Ferguson's volume, *Astronomy explained upon Sir Isaac Newton's Principles.* It soon appeared, his sister Caroline recorded in her journal, 'that my brother was not contented with knowing what former observers had seen.' 'He went to sleep buried under his favourite authors; and his first thoughts on rising were how to obtain instruments for viewing these objects himself of which he had been reading.' In May 1773 he procured some object-glasses which he fitted into pasteboard tubes. But he had no sooner turned these home-made instruments to the heavens than he was brought face to face with the chromatic aberration which had arrested the development of the refracting telescope. Accordingly, he turned his attention to the reflector, which had recently been considerably improved by James Short, an Edinburgh optician. Finding the price of such an instrument to be beyond his means, Herschel conceived the daring idea of constructing a telescope for himself. Astonishing success attended his efforts, and on the 1st of March 1774 he made his first observation with his hand-made instrument. This first telescope, however, did not satisfy him, and accordingly he constructed larger and still larger instruments, with one of which he first saw the planet Uranus, the discovery of which brought him at one leap from obscurity to fame.

After his settlement as King's Astronomer with a residence at Slough, near Windsor, Herschel constructed a number of still larger instruments, the most serviceable of which was the celebrated 20-foot reflector, the mirror of which measured nearly 19 inches in diameter. A still more ambitious venture was the famous 40-foot, with mirror of 4 feet in diameter. This telescope soon took rank as the scientific wonder of the age, but its performance was disappointing, for it was too large and cumbersome to be of much value, more particularly as telescopes were not at the time driven by clockwork.

Herschel died in 1822 in his eighty-fourth year. He found astronomy a sedate elderly science, and astronomers engaged in the task of consolidating the scientific gains of their predecessors. He left the science youthful and vigorous, standing on the threshold of the unknown. It was in all likelihood with a consciousness of the import of the work of Herschel in his mind that Laplace uttered on his death-bed his often-quoted words, 'What we know is but little: what we do not know is immense.'

II

OUR KNOWLEDGE OF THE MOON

It was only fitting that our nearest neighbour in space—our detached continent, as the Moon has been picturesquely called—should have been the first celestial object to be observed through the telescope. In 1609 Galileo turned his small hand-telescope on the Moon and made the discovery, much to the chagrin of the Aristotelian philosophers, that the bright areas were rugged and mountainous highlands. The darker spots were mistakenly assumed by the seventeenth-century astronomers to be seas, and so on the earliest lunar charts these objects were designated by the Latin name of *maria,* which misnomer is still current. During the seventeenth century the first charts of the Moon were constructed by Hevelius at Danzig, and by Riccioli at Bologna. These were followed in the eighteenth century by the more elaborate chart—7½ inches in diameter—constructed at Göttingen by Tobias Mayer. This chart, found among Mayer's papers after his death, was published at Göttingen in 1775, and remained for many years the only trustworthy map of the Moon.

Up to the close of the eighteenth century observations on the Moon had been sporadic, and the work done had been more or less unsystematic, as was indeed the case in all the departments of descriptive astronomy. At the beginning of his career Herschel devoted a good deal of attention to lunar observation, and one of his earliest papers dealt with the height of the lunar mountains. Soon afterwards other fields claimed most of his attention. His general conclusions concerning our satellite's physical condition were summed up in 1794, when he pointed out that 'we perceive no large seas in the Moon, that its atmosphere (the existence of which has even been doubted by many) is extremely rare and unfit for the purposes of animal life : that its elements, its seasons, and the length of its days totally differ from ours ; that without dense clouds (which the Moon has not) there can be no rain ; perhaps no rivers, no lakes'. Nevertheless, in common with most of the eighteenth-century astronomers, he clung to a belief in the habitability of the Moon, holding that its inhabitants 'are fitted to their conditions as well as we on this globe are to ours'.

If Herschel contributed little to lunar astronomy his indirect influence on the progress of this branch was very great. Johann Hieronymus Schröter, a native of Erfurt, near Hanover, and a student of mathematics at Göttingen, became acquainted, through a common interest in music, with the Herschel family. The work of William Herschel inspired him to turn his attention to descriptive astronomy, and in 1785 he acquired one of Herschel's 7-foot reflectors. Like his great contemporary, he was an amateur. By profession a lawyer, he held for many years the post of chief magistrate of Lilienthal, near Bremen, and in his private observatory there his reflecting telescopes were erected. Either by accident or design Schröter chose for his particular field of research the one branch of astronomy which Herschel had more or less neglected. Indeed, the foundation of lunar astronomy, or selenography, as a distinct branch of the science, was due to Schröter's long-continued work, which extended from 1785 until 1813, when the Napoleonic troops under Vandamme sacked Lilienthal and burned the observatory.

Schröter laid the foundation of the comparative investigation of the lunar surface. He devised a new method of measuring the height of the lunar mountains ; he discovered the curious formations known as rills or clefts as well as a large number of hitherto undetected craters. In effect, he was the first to make an intensive study of the Moon's surface. He laboured, it is true, under the handicap of bad draughtsmanship ; and he lacked that faculty of subordinating theory to observation, which was so notable a characteristic in his great contemporary and fellow countryman, Herschel. To him the Moon was a living world, with volcanoes in active eruption and an appreciable atmosphere, of which he believed himself to have detected the effect in a lunar twilight. Indeed, Mädler considered that this preconceived idea impaired the value of his work, inasmuch as he was too eager to register changes on the Moon on somewhat slender evidence. Nevertheless, he laid the firm foundation of the edifice of modern selenography.

Schröter was followed by Lohrmann, a land-surveyor of Dresden, who turned his attention from the survey of the terrestrial to that of the lunar surface. Lohrmann published in 1824 four of twenty-five sections of an elaborate chart of the Moon's surface, but failure of eyesight, three years later, compelled him to abandon his project. About the same time Johann Heinrich von Mädler commenced his monumental work on lunar topography. Mädler had just received a teaching appointment at the close of a university career, which had been made possible by dint of much struggle and sacrifice, when he had the good fortune to be appointed scientific tutor to Wilhelm Beer, a wealthy young banker, a brother of Meyerbeer the composer. Together teacher and pupil commenced systematic work in astronomy, and their most important undertaking was an exhaustive survey of the Moon's surface. The instrument used was not large—a refracting telescope of four inches aperture. But Mädler was a born observer, and, in collaboration with Beer, he measured the positions of 919 lunar formations and the height of 1,095 mountains. Their great lunar chart was issued in four parts during the years 1834-6. It has been said of this chart that 'the amount of detail is remarkable, and the labour actually bestowed on the work will appear incredible'. The chart was followed in 1837 by a descriptive volume entitled *Der Mond: oder allgemeine vergleichende Selenographie* (*The Moon: General and Comparative Selenography*). In this monumental work Beer and Mädler recapitulated the sum of human knowledge concerning our satellite. Their view of the lunar world as changeless, airless, and lifeless was much nearer to the truth than the 'baseless fabrics' of Schröter's visions. At the same time the publication of this exhaustive volume accompanying an equally exhaustive chart tended to discourage further investigation of the Moon's surface. Beer and Mädler were believed to have ascertained all that was worth knowing, and to have established the fact that the Moon was to all intents and purposes dead. Indeed, the view still survives that it is a waste of time for an astronomer to devote himself to lunar research.

One or two observers, however, were undismayed by the generally accepted view that the Moon was a dead world. Of these the chief was Schmidt, the German director of the Greek National Observatory. Schmidt studied the Moon for a much longer period than Mädler; indeed, his great chart, published in 1878, was the outcome of forty years of untiring labour. In 1866 he brought forward strong evidence of change on the Moon's surface, in the almost complete effacement of the small crater Linné. Lohrmann and Mädler had observed and delineated the crater under a totally different aspect, as had also Schmidt himself. This observation, confirmed by other astronomers, was followed by the equally startling announcement by Klein of Cologne in 1879, of the formation of a new crater.

Nevertheless, the majority of astronomers adhered to Mädler's view. Nasmyth and Carpenter, in their classical work on *The Moon*, expressed 'the strong belief that no vestige of its former volcanic activity lingers on the Moon—that it assumed its final condition an inconceivable number of ages ago'; while conceding that 'minute changes of a non-volcanic character may be proceeding in the Moon, arising from the violent alternations of temperature to which the surface is exposed during the lunar day and night'. Flammarion, in 1879, was as a voice crying in the wilderness. Referring to the darkening of the floor of the great walled-plain of Plato with the progress of the lunar day, he expressed the view that 'the odds are ninety-nine to one that it is not the light which produces the effect, and that it is the solar heat. . . . It is highly probable that this periodical change of tint on the circular plain of Plato, visible every month to any attentive observer, is due to a modification of a vegetable nature, produced by the temperature'.

The work of Schmidt was carried forward by a number of painstaking English amateurs, chief among whom was the late Mr. T. G. Elger of Bedford. In 1895 Elger published his reasoned conclusions in a balanced and cautious way in his volume on *The Moon*. His standpoint represented a reaction from the view of Mädler and from the much more extreme version of that view given in so many text-books. 'The knowledge we possess,' he wrote, 'even of the larger and more prominent objects, is far too slight to justify us in maintaining that changes which on Earth we should use a strong adjective to describe have not taken place in connexion with some of them in recent years.' Elger himself inclined to belief in change. Referring to the varying tints of some of the level regions, he cautiously remarked: 'It has been attempted to account for these phenomena by supposing the existence of some kind of vegetation, but, as this involves the presence of an atmosphere, the idea hardly finds favour at the present time, though perhaps the possibility of plant growth in the low-lying districts where a gaseous medium may prevail is not altogether so chimerical a notion as to be unworthy of consideration.' He stated, however, his strong conviction of the need of further intensive study, maintaining that only by confining attention to selected areas of limited extent, and by studying every visible object under all conditions of illumination, could progress be made. 'A complete photographic survey of a few selected regions as a basis for an equally thorough and exhaustive scrutiny by direct observation would, it is believed, lead to a much more satisfactory and hopeful method for ultimately furnishing irrefragable testimony as to permanency or change than any that has been undertaken.'

Elger died in 1897; three years later his suggestion was put into practice by Professor W. H. Pickering, of Harvard, who since the beginning of the century has had the field of lunar astronomy practically to himself. On 12 August 1892 an occultation of Jupiter was observed by Pickering at Arequipa, in Peru, and the slight flattening of the disc was attributed by him to refraction in a lunar atmosphere of extreme tenuity. His observations of the crater Linné during the next few years confirmed him in the belief that the Moon was by no means destitute of change. In 1899 an expedition to Jamaica convinced him of the excellence of its climate for the photographic and visual study of the Moon, and in 1900 the Harvard astronomical station was erected at

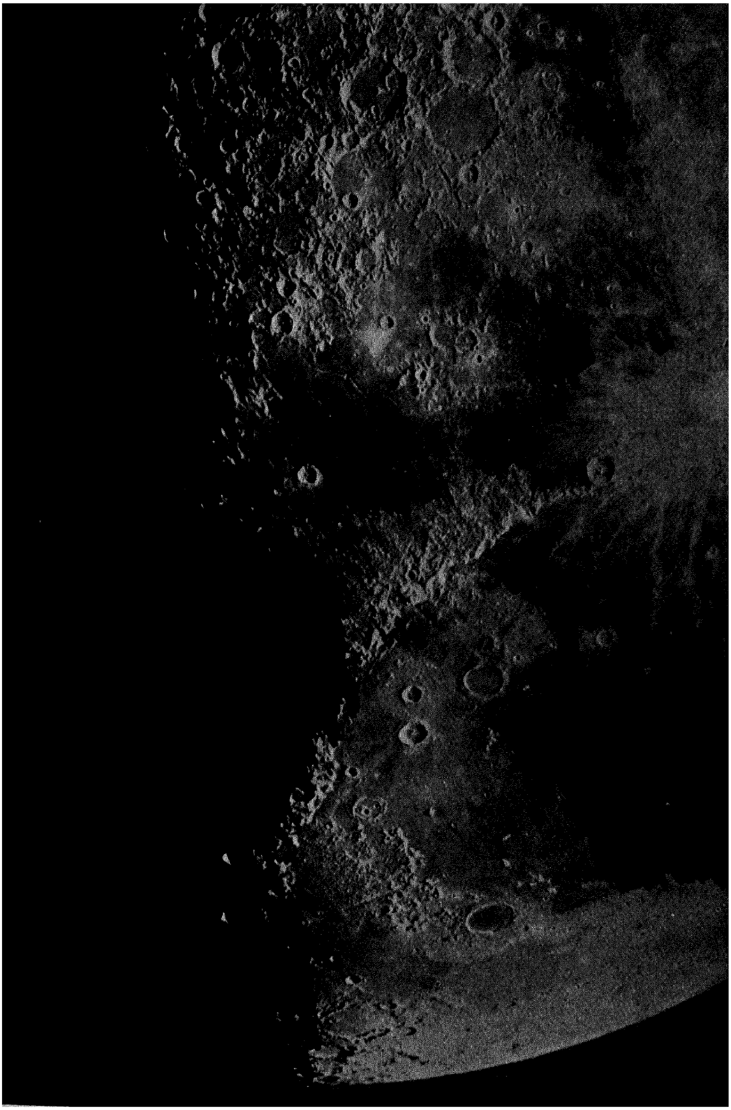

FIG. 6. PORTION OF THE MOON. (*G. E. Hale*)

Mandeville, a convenient point in the island. The photographs of the lunar surface taken during the following year formed the basis of the large atlas, containing many plates and descriptive matter, which was published towards the close of 1903 under the title of *The Moon.*

Pickering was not the first to apply photography to the lunar surface. At the Lick Observatory a large atlas was commenced, but the project was discontinued. At the Paris Observatory, Loewy and Puiseux obtained many beautiful photographs ; while Weinek, of Prague, formed an atlas combining many Paris and Lick photographs. But the amassing of large numbers of isolated photographs is in itself insufficient to settle the vexed question of change, or, indeed, to add much to our knowledge of the Moon. Pickering was the first investigator to carry out the idea of Elger—photography of a few selected regions as a basis for an exhaustive scrutiny by direct observation. The idea has been justified by the progress of selenography since the beginning of the century. Indeed, there has been developed what Pickering himself calls the 'new selenography'—'the selenography which consists not in a mere mapping of cold dead rocks and isolated craters, but in a study of the daily alterations that take place on small selected regions—changes that cannot be explained by shifting shadows or varying librations of the lunar surface'.

A close study of these 'small selected regions' convinced Pickering as early as 1902 that the accepted view of the Moon as an absolutely dead world was untenable. He had concluded ten years earlier that a very thin atmosphere does exist, with a density probably not exceeding one ten-thousandth part of our own, and his later work indicated that this conclusion had been correct. It is obvious that only the heavier gases can be components of such an atmosphere ; the lighter gases must have long since escaped into space. Indeed, Pickering declares that 'any gas that was not constantly renewed from the Moon's interior would have practically disappeared from its surface long ago. Let us now see what gases are at the present time being given off from the Earth's interior. We find that there are only two that escape in large quantities—carbonic acid and water-vapour. The former would remain for some time on the Moon's surface on account of its weight, and the latter because, on account of the low pressure, the rapid evaporation would cause it immediately to freeze.'

These theoretical considerations form the background of Pickering's theory of the lunar surface, where he believes volcanic activity to persist on a small scale, and hoar-frost to be extensively deposited. Those who accepted the evidence of Schmidt, Klein, and others as to changes on the Moon, usually attributed them to the crumbling of rocks due to the extreme range of temperature. Few believed in the existence even of feebly active volcanoes. Accordingly, when Pickering first saw the small crater at the source of a deep winding cleft known as 'Schröter's Valley', he was amazed by its strong resemblance to the crater of a terrestrial volcano in active eruption. Clouds of white vapour, of great density, were seen rising from the bottom of the crater and pouring over its south-eastern wall. Observations at Cambridge, Mass., and at Mandeville, confirmed these early results and showed that this activity commenced a day or two after lunar sunrise, increased to a maximum, and ceased a few days before sunset. Even the few examples which Pickering cited in 1903 were sufficiently striking to lend strong support to the view that lunar volcanic activity had not altogether ceased.

As water-vapour cannot liquefy on the Moon's surface, owing to the low pressure, it must immediately freeze. If so, therefore, one would naturally expect to find evidences of snow or hoar-frost on the lunar surface. This is exactly what Pickering found. He has ascertained that a considerable number of craterlets are lined with a white substance, which becomes more and more brilliant as the Sun shines upon it. The same substance is to be seen on certain mountain peaks.

Many of these features have been detected because Pickering has not confined his studies of lunar formations, as most selenographers have done, to the periods of lunar sunrise and sunset when they are most clearly defined. He holds, indeed, that ‘ the only time when the Moon is interesting to one who is really used to its surface is when changes are taking place on it, and those only occur far from the terminator ; that is, during the lunar summer-time ’.

Round the small crater Linné is a halo of bright material, which becomes visible a day or two after sunrise. Pickering made a series of measurements of this halo, but they were strangely discordant. Indeed, the results seemed inexplicable until it occurred to him

> ‘ to compare the diameters of the area in question with the number of hours that it had been exposed in each case to the Sun. The whole matter then became clear. When the white spot first became visible, one and a half days after lunar sunrise, it was five miles in diameter. As the Sun rose the spot rapidly diminished in size, until one day after the lunar noon it was only two and a half miles in diameter. From then till one and a half days before sunset, when it disappeared, it steadily increased in size, reaching a diameter of four miles. During the lunar night it must have continued to increase, until after sunrise it became, as before, five miles in diameter.’

Soon after these measures were made it occurred to Pickering that a crucial test of the hoar-frost theory would be possible during a total eclipse of the Moon, when the solar rays are cut off by the Earth’s shadow. It was obvious that if the tentative explanation be true, the spot should increase in size during eclipse. During the eclipse of 27 December 1898, Pickering was unfortunately handicapped by bad weather, but Douglass at the Lowell Observatory found a marked increase of size. This observation was confirmed by Pickering himself during the eclipses of 1899, 1902, and 1903, and independently by Mr. S. A. Saunder, the English astronomer, on the occasion of the third of these eclipses.

The visibility of snow or hoar-frost on the Moon is conditioned by two opposing factors. In the first place, the snow tends to melt under the direct rays of the Sun ; while secondly, the snow becomes visible only when illuminated by the sunlight. As the Sun rises higher in the sky, it shines into crevices on the surface and illuminates white regions previously invisible. A striking instance of increasing visibility is that of the bright streaks or ' rays ' surrounding certain prominent craters, more particularly the conspicuous formation known as Tycho. These streaks, which are seen only about the full phase, were a standing puzzle to the older selenographers. Nasmyth supposed them to be due to cracks in the surface filled by an outflow of white material from the interior of the Moon, and in this he was followed by Proctor. Pickering's explanation is much simpler. The snow which gives rise to these ' rays ' lies in crevices, and cannot become visible until the Sun has attained a certain altitude. Less convincing, perhaps, is Pickering's explanation of why snow is deposited in these crevices—by winds blowing in certain directions. That the tenuous atmosphere must have some mode of circulation is manifest, however, from recent investigations in 1917 of the minute craterlets near the formation known as Theophilus. The water-vapour expelled from the crater cone obviously does not freeze as it ascends. There is some form of atmospheric diffusion. ' These changing snow areas seem necessarily to involve the existence of low cloud or fog in some form or another, and thin clouds doubtless account for some of the less brilliant areas observed.' Confirmatory evidence of the existence of fog was obtained by the Italian astronomer Maggini at Florence. On 10 October 1916, while observing Plato, he noted that a minute craterlet on the floor became invisible, while a reddish mist extended over a portion of the floor and also over the rampart. After three-quarters of an hour the mist cleared away, and the craterlet again became visible.

The evidence which Pickering has collected of the existence of some form of vegetation on the lunar surface is pretty strong, if not fairly conclusive. The possibility of the presence of vegetation was mentioned as long ago as 1858 by De La Rue, while Flammarion suggested it as the explanation of the darkening of the floor of Plato. However, the majority of astronomers dismissed the idea as absurd. Unperturbed by the prevailing scepticism, Pickering began as early as 1893 to make close investigation of a number of variable spots, which darkened rapidly after sunrise and faded out towards sunset. A considerable number of these spots have been studied by him, and it is difficult to refute his contention that they represent a rudimentary kind of vegetation springing up and withering during the long lunar day, which is more analogous to our terrestrial year than to our day. Regarding the nature of this vegetation we know nothing. Pickering has pointed out, however, that

> 'lunar vegetation would have two distinct advantages over our own. In the first place, since the force of gravity is less upon the Moon, the same leaves or fronds or branches would require but one-sixth the effort to lift and support themselves that would be necessary were they transplanted to our Earth. Secondly, since there are no high winds upon the Moon, if it were any advantage to plant life to lift itself above the surface of the ground, it could do so with safety, instead of clinging close to the rocks, like our own arctic and antarctic flora.'

It is plainly evident, of course, that the Moon cannot support anything but the lowest forms of vegetable life. The length of the lunar day and night, the tenuity of an atmosphere whose very existence is still denied by some astronomers and the extremes of heat and cold which prevail on the lunar surface would seem to be conclusive on this point. The night side of the Moon is probably about as cold as interplanetary space. As to the illuminated side, the most reliable conclusions are those of Professor Very, the American astronomer. In a letter to the late Dr. Lowell, Professor Very wrote:

> 'When the Sun rises, no matter in what latitude, it is cold. I do not venture to say how cold, but below freezing-point. Not

until the Sun has reached an altitude of 15° in middle latitudes does the temperature get above freezing. Then the heat mounts rapidly until at the end of the first week of sunshine, in dry regions near the equator, the rock surface is as hot as boiling water. As mid-day approaches, the scorching rocks attain a temperature full 80° Centigrade above the boiling-point of water in regions under a vertical Sun (356° F.). Having once become heated, the rocky surface retains its heat to a great extent far into the afternoon, the curve of falling temperature being perhaps a day and a half of our time out of symmetry. Toward the end of the lunar afternoon, the fall of temperature is very rapid, and before the Sun sets, frost prevails, or at least temperatures which produce frost wherever there is water-vapour to make the article which we call hoar-frost.'

In the last century the belief was fairly widespread that the present condition of the Moon represented the future condition of the Earth—that the Moon once had an extensive atmosphere and oceans, and that air and water had disappeared as a consequence of planetary decrepitude. It is very doubtful, however, in the light of the kinetic theory of gases, if the Moon with its small mass was ever possessed of an extensive atmosphere. That the *maria*, or flat plains, represented old ocean-beds was a favourite theory during the past century, but grave doubts have been thrown on it by Pickering's work. In 1916 he pointed out that the extensive lack of areas of erosion clearly indicates that there was never very much water on the surface of the Moon. There are evidences of erosion, but on a very small scale, and this is borne out by theoretical considerations based on the small mass of our satellite and the tenuity of the atmosphere even in early times.

It must be admitted that many astronomers hesitate to accept Pickering's conclusions. In a recent lecture, Professor Aitken, of the Lick Observatory, remarked that 'further confirmatory observations are desirable before we accept these changes as demonstrated; and even then we may well hesitate to accept the explanations that have been offered'. But we must bear in mind that the critics of Pickering's view have devoted no special attention to the surface of the Moon, while Pickering is the one prominent observer who, under favourable atmospheric conditions, has studied the Moon intensively for over a quarter of a century. Naturally, therefore, Professor Pickering's conclusions must be admitted to carry a great deal more weight than the objections of his critics, however eminent these may be in other branches of astronomy. Therefore we may say, with some degree of confidence, that the balance of evidence is on the side of the view that changes do take place on the Moon; in other words, that the Moon is not a dead, but a dying world.

If, as Pickering believes, there are still traces of volcanic activity on the dying Moon, this is a strong argument in favour of the traditional volcanic theory of the origin of the lunar craters. This hypothesis has indeed been challenged from time to time, and various theories have been advanced in opposition. Of these, the only one with any degree of plausibility is the meteoric theory. This theory, which has been advanced from time to time, was adopted by Dr. T. J. J. See, and incorporated in his cosmogony. According to this theory, the craters have been formed by the impact of large numbers of meteors, varying in size from small asteroids to large meteors, on the Moon's surface. A strong argument against this hypothesis is the distribution of the craters on the lunar surface; on the impact theory there ought to be some measure of uniformity of distribution. At the same time, meteoric action may well have played a subsidiary part in the shaping of the Moon's surface. Dr. C. P. Olivier pointed out recently that, owing to the lack of an atmosphere of appreciable density, the result of the fall of meteors on to the Moon's surface has been similar to the effect of erosion. 'The general effect of all this, be it great or small in amount, must be somewhat to smooth the lunar surface. Had it not acted at all, that surface would be even rougher than it now is.'

III

OUR NEAREST STAR—THE SUN

Like the other branches of descriptive astronomy, the study of the Sun had its origin when Galileo first observed it through his ' optic tube '. The great Italian's priority in the discovery of the solar spots cannot indeed be established. It seems likely that the German, Scheiner, and the Dutchman, Fabricius, who possessed telescopes soon after Galileo, made the discovery independently of him and of each other. From observations of these spots, Scheiner found that the Sun rotated on its axis, and he was enabled to determine the period of rotation—about twenty-five days—and the position of the solar axis with a fair degree of precision. The only other important piece of work concerning the Sun which was accomplished in the seventeenth century was Cassini's estimate of its distance as eighty-seven million miles, which was only six million miles short of the modern determinations, and was indeed a remarkable approximation to the truth.

During the eighteenth century, solar investigation languished, and save for the important work of the Scottish astronomer, Alexander Wilson of Glasgow, there was no solar research worthy of the name. Solar astronomy, as a distinct branch of the science, dates from the closing years of the century. William Herschel, amid his multifarious investigations, found time for the systematic study of the solar photosphere,[1] and to him is due the credit of the enunciation, as the outcome of his scrutiny of the dark spots and the bright faculae, of the first 'solar theory' worthy of the name. Its main outlines had, it is true, been drawn by Wilson, but it went out to the world developed and perfected by Herschel, and carrying with it the weight of his authority. Briefly stated, the theory was that the sun-spots were gigantic holes in the 'shining atmosphere', through which the dark solid body of the Sun was to be seen. The 'solid body of the Sun', Herschel concluded to be 'nothing else than a very eminent, large, and lucid planet', which, in accordance with the prevalent, ideas of the eighteenth century, he believed might be 'richly stored with inhabitants'. Although fallacious, the theory deserves to be remembered as the first serious attempt to co-ordinate the isolated facts ascertained concerning the solar globe.

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[1] The ' photosphere ' is the name given to the radiating surface of the Sun.

In the history of solar research, since Herschel's time, three well-defined periods may be traced. The first of these may be called the telescopic period, because during that period the telescope alone, with no auxiliary instrument, was used for observation. In 1826, Samuel Heinrich Schwabe, an apothecary in the small German town of Dessau, began to amuse himself by looking at the Sun from day to day through a small hand telescope. Every clear day Schwabe pointed his telescope at the Sun and counted the number of visible spots—a pastime which he continued for forty-three years. After seventeen years' observations had been accumulated, Schwabe was struck by a certain periodicity in the number of spots visible per day. By 1851, what he had suspected was confirmed beyond all doubt; the number of sun-spots passed through a cycle, increasing and decreasing in about ten years. No one was more surprised at this first-class discovery than Schwabe himself. He compared himself to Saul, who, seeking his father's asses, had found a kingdom.

After Schwabe had detected this periodicity, which earlier observers had declared to be non-existent, Rudolf Wolf of Zurich, the erudite historian of astronomy, searched through all available records of sun-spot observations, from the time of Galileo and Scheiner downwards, in order to check Schwabe's results. The discovery of the obscure apothecary was abundantly confirmed. Wolf's investigation enabled him to determine the average length of the solar cycle, more accurately than Schwabe had done, as 11·11 years. Additional interest was given to this discovery by the simultaneous discovery by Lamont, a Scotsman naturalized in Germany, who presided over the Munich Observatory, that terrestrial magnetic variations obeyed a somewhat similar period. Thus the solar cycle was shown to have a terrestrial counterpart, and the presumption was very strong that this was no coincidence.

In 1852 two amateur astronomers in England and Germany respectively, doubtless inspired by Schwabe's example, took up the study of sun-spots. The movements of the spots had been for two centuries the means by which the Sun's rotation was measured. Carrington and Spörer discovered that while the period of the Sun's rotation is about 25 days near the equator, it is protracted to 27½ days midway between the equator and the poles. Further, Carrington showed in 1858 that spots are scarce just on and near the equator, and are generally confined to two zones on either side. Spörer showed further that the disturbance which gives rise to the spots of any given cycle is first observed in two belts thirty degrees north and south of the equator. The belts draw nearer and nearer to the equator; the sun-spot maximum is reached when the latitude is sixteen degrees north and south, and the disturbance dies out at a latitude of eight or ten degrees. Before the minimum is reached, however, a new outbreak has already manifested itself in high latitudes.

Besides the steady systematic investigation of the photosphere, some desultory work had been done on the outer solar envelopes, then visible only on the rare occasions of total eclipses, when the dark body of the Moon just cut off the photospheric light. The prominences or 'red flames', and the silvery corona had been observed from time to time during the eclipses of the eighteenth century. But the eclipse phenomena were not properly interpreted, and very hazy and rudimentary notions were entertained until the great eclipse of 1842. Observations of that eclipse, as well as those of 1851 and 1860, proved that the corona and the chromosphere[1]—the rose-coloured ring of light encircling the eclipsed Sun, from which the red flames obviously emanated—were without doubt appendages of the day-star.

The second period of solar research, which may be called the spectroscopic, opened in 1859, when Kirchhoff, of Heidelberg, announced the first results of his spectroscopic investigations. A word or two must be said about the principle on which this instrument is based. Newton had discovered in the course of his memorable experiments on light that a ray of light in passing through a prism was dispersed into its component colours, a band of rainbow-tinted light. The rainbow, indeed, may be called a natural spectrum. For many years the spectrum was an object of curiosity rather than of study. Herschel was probably the first to investigate it scientifically, and even he failed to detect its most conspicuous features—the dark lines which were casually seen by Wollaston, an English physicist in 1802, and first thoroughly investigated by Joseph von Fraunhofer about a decade later.

The career of Fraunhofer reads almost like a romance. A little consumptive orphan, he toiled by day for a miserable pittance as apprentice to a sweating employer named Weichselberger, a looking-glass maker in Munich. On the 21st of July 1801 the foetid slum in which the little apprentice lodged tumbled down, burying in its ruins all its inmates. All were killed instantaneously, save Fraunhofer, who was extricated from the debris seriously injured, but still alive. It so happened that the Elector of Bavaria was a witness of the scene, and took a kindly interest in the sole survivor of the catastrophe—presenting him with a sum of eighteen ducats. Part of this purchased his release from his tyrannical employer, and with the remainder he procured books and a glass-finishing machine. After much privation and hardship, Fraunhofer succeeded in receiving an appointment in an optical establishment in Munich, where his talent as a maker of lenses and prisms soon asserted itself, and of which he became in time the head.

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[1] The chromosphere is the solar atmosphere proper, composed of the lighter gases.

In the course of his experiments with the prism, Fraunhofer was astonished to find a number of dark lines in the solar spectrum, since known as the Fraunhofer lines. At first he suspected that these might be due to an optical effect, and he tried different prisms, only to find that they still persisted. Then it occurred to him that they were atmospheric in origin, and this surmise seemed to be confirmed when he found them in the spectra of the Moon and planets. As a final test, however, he observed the spectra of the brighter stars, and he found lines—but lines differing in position and intensity from those in the solar spectrum. The experiment proved conclusively that the lines were solar in their origin. Fraunhofer concluded that the lines were due to absorption of some kind or other alike in Sun and stars. Further, he noted that the bright lines visible in the spectrum of sodium gas coincided in position with the strong dark lines in the solar spectrum to which he had affixed the letter D. Further than this Fraunhofer did not go, for his researches were cut short by his untimely death in 1826, at the age of thirty-nine.

It was not in the optical institution at Munich, but in the laboratory at Heidelberg, that spectroscopic astronomy was born. In the autumn of 1859, Gustav Robert Kirchhoff, then engaged in the study of the spectra of luminous gases, carried through the crucial experiment.

> ‘ In order ’, he said, ‘ to test in the most direct manner possible the frequently asserted fact of the coincidence of the sodium lines with the lines D, I obtained a tolerably bright solar spectrum and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines D change into bright ones. . . . In order to find the extent to which the intensity of the solar spectrum could be increased without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine through the sodium flame, and to my astonishment I saw that the dark lines D appeared with an extraordinary degree of clearness.’

Shortly afterwards, Kirchhoff announced the general principles on which spectroscopy is based. A luminous solid or liquid gives a continuous spectrum, and a gaseous substance a spectrum of bright lines. Further, he established the law that substances of every kind are opaque to the precise rays which they emit at the same temperature; they stop the rays which they are in a condition to radiate. The dark lines of the spectrum then are due to the existence in the solar atmosphere of many of the chemical elements with which we are familiar on this planet. Kirchhoff, at an early stage, announced the existence in the Sun of such familiar elements as sodium, magnesium, iron, calcium, nickel, copper, and zinc.

The spectroscope was applied in the first instance to the analysis of the dark lines in the photospheric spectrum. These lines have their origin in the gaseous layer just above the photosphere, and appear dark only by contrast. During the total eclipse of 22 December 1870, which he observed in Spain, Young, of Princeton, was able to get a glimpse of the spectrum of this lower atmospheric layer seen by itself. As the solar crescent grew apparently thinner before the advancing disc of the Moon, 'the dark lines of the spectrum', said Young, 'and the spectrum itself faded away until, all at once, as suddenly as a bursting rocket shoots out its stars, the whole field of view was filled with bright lines, more numerous than one could count'. To this lower layer, in which the Fraunhofer lines have their origin, Young gave the name of the 'reversing layer'. Confirmatory observations of this 'flash spectrum', as the phenomenon of reversal from dark to bright was called, were secured at subsequent eclipses, and in a photograph of the flash spectrum, taken at the 1896 eclipse, Young identified the majority of the bright lines as simply reversals of the dark, while about twenty-five, much more conspicuous than the others, were believed by him to be images of the chromosphere and prominences. The actual existence of the 'reversing layer' was disputed by many astronomers, chief among them Sir Norman Lockyer, who regarded the dark lines as due to the whole of the outer envelopes. But on the whole, Young's conclusions were substantiated with the progress of research.

The spectroscope was first used for the study of the chromospheric envelope proper by Lockyer and the French astronomer Janssen, working independently in 1868. The English astronomer detected a line which could not be ascribed to any known terrestrial element. The unknown element which was called helium, and is now known to exist on the Earth, is one of the chief constituents of the chromosphere. After the total solar eclipse of 1868, the two astronomers found that by using a high dispersive power, and so weakening the ordinary solar spectrum, they were able to observe the bright

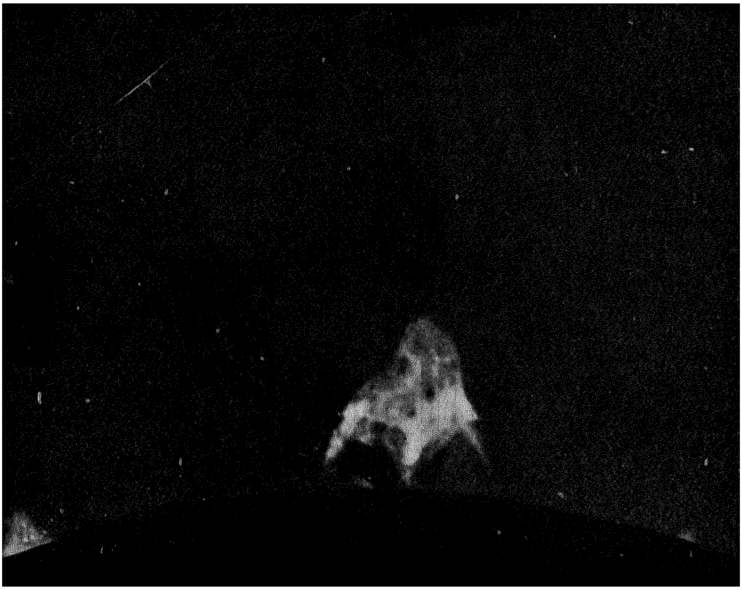

FIG. 7. QUIESCENT AND ERUPTIVE PROMINENCES

Photograph by Yerkes Observatory

prominence-lines in full daylight. Shortly afterwards, Huggins and Zöllner showed that by widening the slit of the spectroscope they could see not only the lines of the prominences, but the mysterious 'red flames' themselves. The persistent and systematic study of the prominences was carried on by a school of eminent Italian astronomers, who did for the prominences what Schwabe, Carrington, and Spörer did for the spots. These were Secchi—better known for his work in stellar spectroscopy—Respighi, Tacchini, and Riccó. The labours of these men established the fact that the prominences obey a cycle of change almost identical with the sun-spot period. Tacchini showed that the long streamers of the corona originated in the regions where prominences were most prevalent, and that the shape of the corona varied in harmony with the solar cycle.

Perhaps, however, the application of the spectroscope to the measurement of motion was the most important development during this period. In 1842, Christian Doppler of Prague pointed out that, theoretically, the colour of a luminous body should be changed by motion of approach or recession, just as the sound of a sonorous body is altered. The change in colour, however, is so slight as to be imperceptible; what actually happens is a slight shift of the entire spectrum in one direction or another. There is, therefore, a displacement of the spectral lines in one direction—towards the violet—if the source of light is approaching, and in another direction—towards the red—if it is receding. The application of Doppler's principle to the measurement of the rotation and atmospheric motions of the Sun was one of the epoch-making advances in this period. In the hands of Dunér, the Swedish astronomer, the spectroscope confirmed the discovery by Carrington and Spörer of the Sun's equatorial acceleration, and indicated that its rotation period was protracted to about thirty-eight days near the poles. Measures of the velocities of prominences were also secured by Young and others.

In 1891, the third period of solar research was inaugurated.

It may be called the spectroheliographic period, from the instrument which in that year became the invaluable adjunct to telescope and spectroscope. The spectroheliograph was devised independently by Professor George E. Hale, afterwards director of the Mount Wilson Observatory, in California, who was then in charge of a private observatory near Chicago, and by M. Henri Deslandres, of Meudon, near Paris. The principle of the spectroheliograph has been lucidly explained by Professor R. A. Sampson, in his admirable little monograph, *The Sun*, in the following words :

> In a spectrogram, or photograph of a spectrum, ' each line is a record of the presence and the state of a separate chemical element at the spot of the disc to which the slit is directed. If this record could be read for that special line for the whole disc, we should have the same information summed up for the whole Sun. . . . Let the light from the line in question be allowed to pass to the photographic plate, by means of a second slit, at the focus of the camera, the jaws of which shut off all the rest of the spectrum. Let both the first and the second slits be long enough to extend right across the image of the Sun. Move the image of the Sun across the first slit, then the light which passes through the second slit will come at every moment from different strips of the Sun's surface ; and if the photographic plate be moved behind the second slit, in unison with the movement of the Sun's image across the first slit, a record will be given, not of the radiations of every substance mixed together, as in ordinary photographs or visual observations of the Sun's disc, but of the states of some isolated substances as hydrogen or calcium, and even of different strata of these.'

The early work of both Hale and Deslandres was largely experimental. Both astronomers were handicapped by limited opportunities and instrumental means. In 1908, however, on the assumption by Deslandres of the post of director of the Meudon Observatory, that institution was thoroughly re-equipped, and the director began the construction of a large spectroheliograph ' to reveal the superior layers of the chromosphere by the aid of the light of calcium and hydrogen '. In addition to the spectroheliograph, Deslandres devised an instrument which he called the ' spectro-enregistreur des vitesses '—the velocity-recorder. With this form of spectroheliograph it is possible to determine, by means of Doppler's principle, the radial velocities all over the disc, indicating which vapours are ascending and which are descending. As a result of the work of Hale, Deslandres, and other observers—notably those working under Hale at Mount Wilson—it is now possible to speak as Deslandres has done of the ' progressive revelation of the entire atmosphere of the Sun '.

The last quarter of a century has been a period of unparalleled progress in solar astronomy. The new methods, for instance, have thrown a flood of light on the nature of the solar spots. The telescope, unaided by its modern adjuncts, however, was of little use for fathoming the mystery of the constitution of the spots. For many years Wilson's theory of the nature of the spots was widely accepted. On this hypothesis, which was adopted by Herschel, they represented depressions below the surface of the solar photosphere. Careful observation in the latter half of the last century, however, led a number of competent investigators first to doubt and latterly to challenge the accuracy of the theory. In 1904 the late Professor Young cautiously pointed out that ' as regards the sun-spots, it seems no longer safe to assume that they are always depressions in the photosphere '. More definite was the conclusion reached by Dr. Abbot in 1911, that ' it seems most probable that the level of the sun-spot phenomena seen by ordinary observers differs very little, if at all, from that of the surrounding bright surface of the Sun '.

By 1903 the development of the spectroheliograph permitted the photography of hydrogen clouds as well as calcium. In the spring of 1908 Hale, at Mount Wilson, made the interesting experiment of photographing the Sun in the red light of hydrogen. These photographs revealed whirling storms in a region of the solar atmosphere higher than that of the calcium and lower hydrogen clouds. This gave strong support to the hypothesis of the vortical nature of sun-spots. 'We know now', said Hale in November 1908, 'that they are caused by vortices in the solar atmosphere, and the various theories which do not recognize this fact may be laid aside.' At that time Hale inclined to the view that 'in the case of sun-spots we see a phenomenon somewhat analogous to a tornado or waterspout on the Earth. If the gases at high levels whirl with sufficient velocity, they develop a tube-like extension which reaches down through the comparatively undisturbed gases at lower levels. At the centre of the storm the expansion of the gases due to their rapid rotation cools them,

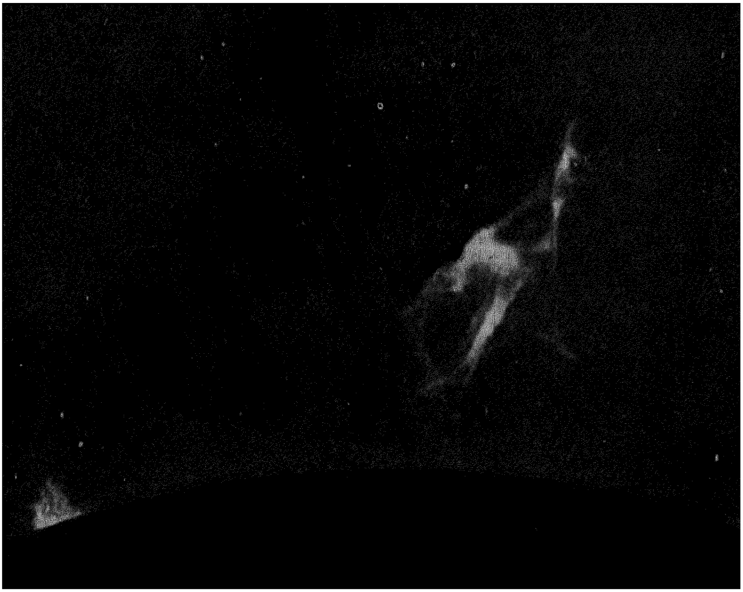

Fig. 8. QUIESCENT AND ERUPTIVE PROMINENCES

Photograph by Yerkes Observatory

and thus produces a comparatively dark cloud which we see in the sun-spot.' At that time Hale believed the disturbances giving rise to the spots to have their origin in the upper regions of the solar atmosphere. This view he modified later.

Experiments conducted by Rowland at Baltimore as far back as 1875, indicated that if electrically-charged bodies existed at all in the solar atmosphere, their revolution in sun-spot vortices would produce a magnetic field; and in 1896 Zeeman, of Amsterdam, showed that the light from a luminous vapour is altered in a certain way when under the influence of a strong magnetic field—the lines of the spectrum being widened or broken up into several constituents. This is known as the Zeeman-effect. With the aid of the 30-foot spectroheliograph of the Mount Wilson Observatory, Hale, in 1908, closely scrutinized the spectra of sun-spots for traces of the Zeeman-effect, and his scrutiny was soon rewarded by the discovery that the double and triple lines in these spectra are exactly similar to the lines produced by the presence of a magnetic field. In the annual report of the Observatory for 1909, Hale was able to announce that the existence of magnetic fields in sun-spots 'has been placed beyond doubt through the detection of all the characteristic phenomena of the Zeeman-effect. As the only known way in which such magnetic fields could be produced at the temperature of the Sun, it thus appears probable that sun-spots are electrical vortices.'

In 1909 Mr. John Evershed, the able English astronomer who directed the observatory at Kodaikanal, in India, detected displacements of lines in the sun-spot spectra, which indicated, on Doppler's principle, the existence of large radial motions in spots—the movement of gases outward from the centre of the spots and parallel to the photospheric surface. In the summer of 1910, Dr. C. E. St. John, at Mount Wilson, commenced a more elaborate investigation, and after two years he confirmed Evershed's discovery, and put forward a hypothesis of its significance. 'We are dealing', he said, 'with an actual flow of the material of the reversing layer out of spots and of the chromospheric material into spots.' In the light of these disclosures, Hale's original sun-spot hypothesis was obviously untenable, or at least only partially true. Accordingly, in 1912, he outlined what he called a 'tentative working hypothesis' as 'a guide to further research'. According to this theory, a column of gas moves upward from the interior of the Sun towards the surface of the photosphere. Owing to differences of velocity of adjoining surfaces or irregularity of structure, a vortex motion is set up. The circulation in the vortex is vertically upward and then outward. As a result of expansion in the central portion of the vortex, cooling sets in, and a comparatively dark cloud—the sun-spot umbra—is formed. 'A rapid flow of negative ions sets in towards the cooler gases at the centre from the hotter gases without. These ions, whirled in the vortex, produce a magnetic field.'

Perhaps the most striking of Hale's subsequent discoveries was that of invisible sun-spots, announced in 1922. Most sun-spots, as Hale pointed out, 'are associated in pairs, of opposite magnetic polarity'. Hale concluded, therefore, that single spots are single only in appearance, the visible spot being associated with an invisible spot 'in which the cooling due to expansion is insufficient to cause perceptible darkening of the Sun's surface'. It occurred to him that it might be possible to pick up these invisible spots by means of the Zeeman-effect, and a systematic search was rewarded by the discovery of two invisible spots in November 1921.

A further discovery by the same astronomer followed a long-continued investigation of magnetic polarities in sun-spots. In 1918 Hale wrote: 'We already know that the preceding and following spots of binary groups with few exceptions are of opposite polarity, and that the corresponding spots of such groups in the northern and southern hemispheres are also opposite in sign.' In 1912 Hale had noticed that the spots of the new solar cycle showed different polarity from the spots of the old cycle. This suggested to him, on the vortex theory, that the direction of the whirl within the spot is reversed at the beginning of a new cycle. He waited expectantly for the beginning of the new cycle in 1923; as he surmised, there was again a reversal of polarity. 'The sun-spot period, if defined in the usual way, as representing the variation in the number or total area of all spots on the Sun, is about 11·1 years. But if we regard the period as the interval between successive appearances of spots of the same magnetic polarity, the present results indicate that it is twice as long.'

The systematic study of the chromospheric regions—comprising prominences, faculae, and the higher levels of the solar atmosphere—has been carried on in recent years by Deslandres, Evershed, and the Mount Wilson observers. Evershed's work on prominences has supplemented the earlier work of Tacchini and the Italian spectroscopists. From 1890 to 1906, observing in England, Evershed recorded 11,000 prominences, and from the commencement of his work in India until 1917, 60,000 were numbered and investigated. Evershed confirmed the conclusion of the Italian astronomers with regard to the general identity of the prominence-period with the sun-spot period. He distinguishes two classes of prominences—'large high prominences' and 'prominences associated with sun-spots'. The large and massive prominences are not found directly above spots, while prominences of the second type are invariably associated with spots. The application of the spectroheliograph has now made possible the observation of prominences not only on the edge of the Sun, but all over the disc. Allied to the prominences are the filaments and alignments in the higher regions of the solar atmosphere to which Deslandres has devoted much attention since 1906. These form a kind of network over the disc. Again and again, Evershed has noticed that at a point of the limb where a prominence is visible, a filament or alignment usually ends. Indeed, he identifies filaments with prominences projected on the disc of the Sun. Prominences, alignments, filaments, faculae, and flocculi would appear to be different varieties of the same species, at different heights and of varying degrees of permanence. Undoubtedly the most ephemeral of all solar phenomena are the hydrogen ' bombs ' first noticed by Mr. Ellerman at Mount Wilson on 21 September 1915. The average duration of these is from one to three minutes, and on rare occasions from five to ten. ' The appearance of the phenomenon ', wrote Ellerman in 1917, ' indicates something in the nature of an explosion, in which hydrogen seems to be the only element playing a part. . . . The regions where the bombs are likely to appear are around and among active spot-groups, especially groups which are developing and composed of many members.'

Twentieth-century study of the Sun supplies a tolerably comprehensible skeleton outline of the make-up of the day-star. Right at bottom of the atmosphere is the shining surface which we call the photosphere. The word ' surface ' must, of course, be used with reservation : it is indeed safer not to speak of the photosphere as a surface, but rather, as Professor Dingle does, as a ' discontinuity '. ' The sudden change from a bright continuous spectrum to a much fainter line spectrum ', he cites as ' conclusive proof ' that there is a ' sharp discontinuity at the base of the Sun's atmosphere '. Young held throughout his career that the photosphere consists of an envelope of clouds formed by the condensation of the solar vapours cooled by radiation into space. ' The photospheric clouds are, of course, suspended in the surrounding gases and uncondensed vapours, just as clouds float in our own atmosphere.' But the balance of evidence is in favour of Abbot's contention that ' the Sun, excepting perhaps in sun-spots, is wholly gaseous or vaporous '. Abbot has adduced strong evidence for the conclusion that what gives rise to the appearance of the photosphere is the light from the whole gaseous globe of the Sun. Condensation, Abbot holds, is quite impossible at a temperature which exceeds 6,000° absolute Centigrade. However, he concedes 'it may readily be supposed that some regions of the gas are a little hotter than others, and that these differences of temperature will give rise to differences of brightness'. Abbot accordingly accepts the view of the late Dr. Scheiner, that the bright grains of the photosphere are the wave-crests of gaseous currents of different temperatures.

Dr. St. John's study of the disturbation of the elements in the solar atmosphere indicates that just above the photosphere is the lower solar atmosphere, 'the region including the lower levels of the reversing layer'. This is 'the region of tremendous disturbances in which the upper portion of the sun-spot vortex is located in which occurs the outflow of material from the interior of the Sun and where more or less mixing must occur'. In this region 'occur the vapours of all the elements whose lines appear in the solar spectrum'. Above this lowest level is the general reversing layer whose normal condition is more or less stable. Higher still is the chromosphere which Dr. St. John believes to be 'quite sharply distinguished from this region, both in its composition and in the movement of its constituent gases near spots'. The chromosphere is composed chiefly of the lighter gases, hydrogen and helium and ionized calcium. The presence of calcium, a heavy element, in the chromosphere, was for long difficult of explanation, but the work of the Indian physicist, Saha, on ionization has explained the apparent anomaly. The calcium lines in the chromospheric spectrum are due to the ionized atoms of calcium, which will reach much higher levels than a neutral atom of the same element. The prominences, as already remarked, are the chief features of the chromosphere, and in recent years several prominences have been observed rising to great heights. For instance, the great eclipse prominence of 1919 ascended to a height of 470,000 miles. But this was surpassed by a still higher specimen, observed by Mr. O. J. Lee of the

Yerkes Observatory in 1920. This flame attained an elevation of 516,000 miles.

Above the chromosphere is the corona, the outermost envelope of all. This envelope, or rather, appendage, is always screened from view, except on the rare occasion of a total eclipse of the Sun. Many attempts have been made, by various methods, to photograph the corona in daylight, but these have been so far without success. At an early stage in solar research it became apparent that the shape of the corona is governed by the solar cycle. At maximum the coronal radiance is equally distributed all round the Sun's disc, while at minimum long streamers from the equatorial zones make their appearance. The study of the corona has made much less progress than that of other solar appendages, and accordingly its nature is still largely a matter of conjecture. The coronal spectrum was long ago noticed to be compound, indicating that it shines partially by reflected light from the photosphere and partly by its own inherent light. It has been generally conceded that electrical action has much to do with the nature of the corona, though Arrhenius and others have insisted on the part which radiation-pressure plays in its production. Professor Perrine, in 1918, expressed the view that the matter composing the corona, with the exception of the portion which gives a bright-line spectrum, exists in the form of small solid and liquid particles, and that this matter shines by its own radiance, while the outer corona shines by 'reflected photosphere light'. The greater part of the motions within the corona, according to Perrine, is due to mechanical causes, explosions, light-pressure, and gravity, while he holds that the view that the arrangement of the coronal streamers results from magnetic and electrical causes is not improbable. During the total solar eclipse of 8 June 1918, the Lick Observatory expedition, headed by Professor Campbell, secured a number of photographs which revealed arches of coronal matter surrounding the principal prominences, indicating the probability that the structure of the corona is largely dependent on the forces producing the prominences. That these forces are in great measure electrical and magnetic admits of little doubt.

In two papers, published in 1904, Mr. E. W. Maunder, of Greenwich, brought out some significant facts concerning the connexion between the sun-spots and terrestrial magnetism. In the first paper he showed that, 'of the nineteen great magnetic storms recorded at Greenwich from 1875 to 1903, all the nineteen, without exception, took place when there was present on the Sun a group of spots with projected area of over 1,000 millionths of the area of the whole disc; or when a group at one time very large had returned in a diminished form to the central meridian'. In his second paper, which has been said with justice to establish 'an entirely new conception of the solar action in producing our magnetic disturbances', Maunder showed that the cause of the magnetic disturbances is associated with limited areas on the solar surface. The magnetic action, he finds, does not radiate equally in all directions, but along definite and restricted lines. Thus, stream-lines from many spots may miss our Earth altogether, and thus a great spot need not necessarily be accompanied by a magnetic storm. Maunder found an analogy to these hypothetical stream-lines in the long rays of the corona; and while not committing himself definitely, evidently inclined towards the view of Arrhenius that the streams consist of droplets formed by condensation in the solar atmosphere negatively charged and driven away by radiation-pressure. This hypothesis he spoke of as 'entirely consistent with the appearance of the coronal photographs and with the conditions indicated by the magnetic storms'.

The problem of the Sun's rotation has engaged the attention of astronomers ever since the equatorial acceleration was discovered independently by Carrington and Spörer in the mid-nineteenth century. The classical research of Dunér, completed in 1891, afforded the first spectroscopic determination of the rotation period, by means of Doppler's principle.

Besides confirming the telescopic results of Carrington and Spörer, he measured the spectral displacements up to within fifteen degrees of the poles, and brought out the surprising fact that the rotation period is there protracted to 38½ days.

The second spectroscopic determination of the solar rotation was made in the years 1901–3 by the German astronomer Dr. Jacobus Halm, then assistant at the Royal Observatory, Edinburgh. The result was in the main confirmatory of that of Dunér. The subject was taken up at Mount Wilson soon after the opening of the Observatory, and in 1906 and 1907 Dr. W. S. Adams made his first determination. In lower solar latitudes the measured velocities agreed closely with those of Dunér and Halm ; in the higher latitudes they were intermediate between those obtained at Upsala and Edinburgh. A second determination was made in 1908 and a third in 1911. The chief feature of this third investigation was the study of different levels of the solar atmosphere, and the result brought out in 1911 was surprising and suggestive. The higher strata were found everywhere to be rotating faster than the lower. The law of rotation which satisfied all the observations of Adams indicated that the velocity increases and the equatorial acceleration decreases with increasing distance from the solar surface, or increasing height in the solar atmosphere. Shortly afterwards, in 1913, the observations made at Kodaikanal in India, under the direction of Evershed, hinted at a relation between high-rotation speeds and spot-development, swifter rotation corresponding with sun-spot maximum. But this is not altogether substantiated by the conclusion reached by Plaskett at Ottawa and confirmed by the Mount Wilson observations, of a progressive diminution of the rate of rotation since 1906. The reason for the peculiarities of the solar rotation is still quite unknown, although many surmises have been made. Dunér referred to the problem as ‘ one of the most difficult problems in astrophysics ’. One of the most attractive theories is that which regards the anomaly as a ‘ legacy from chaos to cosmos ’—a survival from the distant past.

Equally obscure is the essential meaning of the solar cycle. It is not impossible that it may find its explanation in the fact, which has from time to time been suspected, that the Sun is in reality a variable star with a very small range of variation in an eleven-year period. Within recent years investigations by Dr. C. G. Abbot, the distinguished American astronomer, have given much support to this idea. Many circumstances conspire to render the determination of what is called the ‘ solar constant ’ a very difficult process, and the original values were very much at variance. The first real pioneering work in this line of research was done by Dr. Langley of the Smithsonian Institution at Washington. He devised an extremely sensitive heat-measurer named the ‘ bolometer ’, and with it he explored the solar spectrum—visible and invisible—from end to end. The observations were made from the top of Mount Whitney, in Southern California, 15,000 feet in height, in order to eliminate as far as possible complications due to the Earth's atmosphere. Langley's first measure of the solar constant has been corrected by the work of his assistant and successor, Abbot, both at Mount Whitney and Mount Wilson. Abbot gave in 1911, 1·95, and in 1915, 1·93 calories per square centimetre per minute as the measure of the constant. ‘ Expressed in another way, the measures indicate that if the Sun's rays could be completely exposed to melt ice exposed continuously to them at right angles, they would suffice to melt a layer 426 feet thick in a year.’

Towards the close of his career, Langley was convinced that the fluctuations in the value of the constant indicated a variability of the solar radiation, and Abbot, as the result of careful research at Mount Wilson in 1905 and 1906, concluded that the range of values of the solar constant indicated a real variability of the solar radiation outside of and not dependent on the terrestrial atmosphere. Since then, Abbot has confirmed this result by further observations and measurements.

If this conclusion is correct—and confirmation from other sources has been forthcoming—then the Sun may be called a variable star.

The impression made on the terrestrial observer by the Sun is wellnigh overwhelming. The great volume of the Sun, over a million times that of the Earth, its enormous mass, its almost incredible temperature of many millions of degrees in the interior, and the tremendous forces which are at work within are assured facts calculated to astonish the astronomer —familiar as they are to him—whenever he stops to consider their import.

IV

THE DWARF PLANETS

THE eight primary planets of the Solar System fall into two well-defined groups. The bodies composing them differ in distance from the Sun, in size, and in physical constitution. The Earth and its three near neighbours are comparatively close to the Sun, and for that reason have been called the inner planets, while Jupiter and the worlds beyond it are naturally referred to as the outer planets. More striking than the difference in distance from the Sun is the difference in size between the two groups; so pronounced is the difference that the inner planets may be designated the dwarfs, and the outer, the giants.

The study of the three dwarf planets forms a department apart. In this branch of descriptive astronomy we are studying our Earth's neighbour worlds, worlds which, differing in many points of detail from the Earth and from one another, are at least members of a homogeneous family of solid globes. The astronomer sees on their discs, dimly in the case of Mercury and Venus, distinctly in the case of Mars, permanent markings which are in striking contrast to the cloudy canopies of the outer planets.

Mercury and Venus, though near neighbours, are very unfavourably placed for observation. They are interior planets, revolving round the Sun in orbits within the orbit of the Earth; consequently, when they come nearest to our world, their dark hemispheres only are exposed to our view, and so astronomers have to be content to observe them when crescent or gibbous in shape. In addition, neither of these planets can be studied, as Mars and the giants can, for hours at a stretch. Mercury, indeed, is rarely visible at all, and it is no small tribute to the care with which the prehistoric astronomers watched the skies, that this small world—the smallest primary planet of the Solar System—was detected at all, twinkling now in the sunset twilight, and now before dawn.

Schröter, the virtual founder of selenography, was also the pioneer of observations on Mercury. The problem which exercised his special attention was that of the planet's rotation period—the length of its day. In 1800 he noticed that the southern horn of the crescent presented a blunted appearance, which he attributed to the existence of a mountain whose height he estimated at eleven miles. His observations of this mountain led him to conclude that the planet turned on its axis in about 24 hours 4 minutes, a conclusion to which his later observations seemed to give countenance. These observations, however, never inspired much confidence, and Schröter's rotation period figured in text-books simply because no other effort had been made to solve the problem, nor indeed to study the small planet systematically.

In 1882 Schiaparelli, the director of the Brera Observatory in Milan, commenced a series of observations on Mercury, extending over seven years. Instead of contenting himself with brief glimpses of Mercury in the sunset and sunrise twilight, he tried the experiment of observing the planet by day, and thanks to his exceptional powers as an observer, to a good telescope and a clear sky, the experiment was justified. Schiaparelli's conclusions were announced on

8 December 1889. He found the faint markings visible on the planet virtually fixed in position, indicating that whatever the length of the day, it was very much longer than ours, and his final conclusion was that, like the Moon, Mercury's period of rotation synchronized with its period of revolution, which means that one hemisphere basks in perpetual day, while the other is condemned to everlasting night. Schiaparelli pointed out, however, that owing to the considerable libration to which the planet is subject—the result of uniform motion on its axis and irregular motion in its orbit—there is sunrise and sunset over a small zone of the surface. As Lowell puts it:

> 'The Sun does not indeed stand stock still in the sky, but nods like some huge pendulum to and fro along a parallel of latitude. In consequence of libration the two great domains of day and night are sundered by a strip of debateable ground 23½° in breadth on either side, upon which the Sun alternately rises and sets. Here there is a true day, eighty-eight of our days in length from one sunrise to the next.'

Schiaparelli concluded Mercury to be a much spotted globe enveloped in a tolerably dense atmosphere. These conclusions of Schiaparelli were in the main confirmed by Dr. Percival Lowell in a series of observations at Flagstaff, Arizona, during 1896 and 1897. Lowell's determination of the rotation period as 88 days was in exact agreement with that of the Italian astronomer. But he saw no signs of an atmosphere of any kind. 'The surface of Mercury', he concluded, 'is colourless, a geography in black and white.' He detected numerous narrow, irregular dark markings; 'cracks', he remarked, 'best explains their appearance and probably their nature'. Lowell concluded Mercury to be destitute of atmosphere, water, or vegetation; 'the bleached bones of a world; that is what Mercury seems to be'. This conclusion was in harmony with certain photometric results reached in 1874 by Zöllner, who found its surface similar to that of the Moon in reflective power.

Venus is our nearest planetary neighbour, and approaches the Earth at times to within 26 millions of miles. Nevertheless, our knowledge of the planet, which has been called the Earth's twin sister, is little more extensive than our knowledge of Mercury. The first telescopic observation of the planet revealed to Galileo the existence of phases similar to the Moon, but his small telescope showed no surface detail. Cassini, observing Venus in 1666, noticed certain dark markings, and his observations led him to the conclusion that the day was little more than half an hour shorter than ours—23 hours 21 minutes. This was confirmed to within a few minutes by Schröter in 1788 and again in 1811. Thirty years later, Di Vico, the director of the Observatory of the Collegio Romano, came to an almost identical conclusion and announced further that the axis of the planet was inclined at an angle of fifty-three degrees to the plane of its orbit. These results, however, did not inspire much confidence; for Herschel, by far the greatest observer of his day, was quite

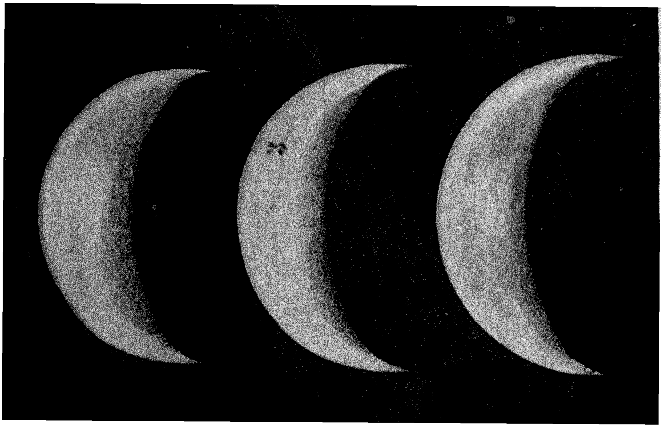

FIG. 9. VENUS IN 1881

From a drawing by Mr. W. F. Denning

unable to see the spots which Schröter had used to determine the rotation period. The diurnal motion, he said, ' on account of the density of the atmosphere of this planet, has still eluded my constant attention so far as concerns its period and direction '.

In 1877 Schiaparelli turned his attention to the problem, and for a period of thirteen years he kept Venus under observation, following it for long periods in the daytime. His conclusion was that, as in the case of Mercury, the period of rotation synchronized with that of revolution, namely 225 days. This indicated that Venus turned one face constantly toward the Sun, and that in one hemisphere there is perpetual day and in the other everlasting night. This conclusion was confirmed in 1895 by a second series of observations by Schiaparelli himself, and by Perrotin at Nice, Tacchini at Rome, Cerulli at Teramo, and in addition by Lowell at Flagstaff. Nevertheless, other competent observers, including Flammarion, were satisfied that the rotation was performed

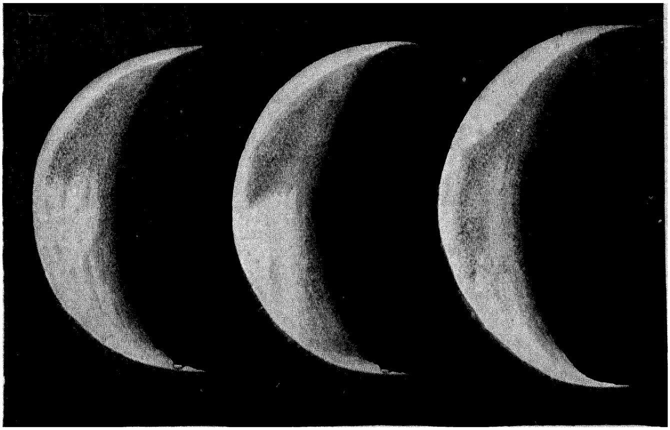

FIG. 10. VENUS IN 1881

From a drawing by Mr. W. F. Denning

in a period of about twenty-four hours. About the beginning of the century the spectroscope was applied to the solution of the problem. By means of Doppler's principle, Bélopolsky, the Russian astronomer, arrived first at a period of 12 hours, and later of 24 hours, while Lowell and his assistants at Flagstaff found that 'the evidence of the spectroscope is against rotation of short duration, and so far as its measure of precision admits, the investigation confirms a rotation of 225 days'.

In 1921 Professor W. H. Pickering announced the results of his observations on Venus during the favourable appearance of the planet in the previous winter. In December 1920 Pickering detected two dark spots near the terminator, and by following these spots—their appearances and disappearances—he reached the conclusion that the planet rotates in a period of 68 hours, and that the axis of rotation lies very nearly in the plane of the planet's orbit. But this is not confirmed by the more recent work of the Mount Wilson observers. Dr. St. John remarked in July 1925 that 'there appears to be no evidence of rotation. . . . As far as our observations go, there is no evidence that the rotation period is less than about fifteen days. A period of a day would be easily detectable by spectrographic observations.'

That dark markings and dusky shadings appear from time to time on the dazzling disc of Venus admits of no doubt. It is, however, difficult to speak with any confidence as to their nature. They may represent irregularities in the surface, as has been generally maintained, or, as Pickering suggests, they may be due to breaks in the cloud-laden atmosphere. White spots have been suspected from time to time, and occasionally brilliant markings have been seen, and interpreted as polar caps or as the snow-clad summits of mountains. That Venus has a dense atmosphere, astronomers are agreed. Definite evidence of this atmosphere was obtained by Schröter in 1792, and it would seem to be considerably more dense than the Earth's aerial envelope. Early spectroscopic observations by Tacchini, Riccó, and Young, during the transits of Venus in 1874 and 1882, indicated the existence of water-vapour in the planet's atmosphere. But the more recent work of Dr. St. John and Dr. Nicholson at Mount Wilson, and of Dr. V. M. Slipher at the Lowell Observatory, has led to negative results; though Dr. Slipher reminds us that 'we would not now be warranted in concluding from these negative results that the atmosphere of Venus does not contain water-vapour'. The Mount Wilson observers have been led to the surprising conclusion that the temperature of Venus is just about freezing-point, that is to say, much lower than the temperature of Mars.

The study of Mars, as of the other planets, dates from 1610, when Galileo first turned the newly-invented telescope on our neighbour world. His sole discovery was the Martian phase; the little red disc refused to give up any of its secrets to the tiny 'optic tube' with which Galileo observed. It was not until 1638 that Fontana, another Italian astronomer, caught glimpses of dusky markings. One of these, now known as the Syrtis Major, was sufficiently well seen by Huyghens to enable him on 28 November 1659 to make the first drawing of the planet. Further, his careful observation of the Syrtis led the Dutch astronomer to the view that the rotation-period of Mars was about twenty-four hours in length. This, however, was never more than a strong suspicion to him, and it was reserved for G. D. Cassini, in 1666, to fix the length of the planet's day at twenty-four hours forty minutes, which was a remarkably accurate determination, in view of the rudimentary state of the telescope at the time. At the very favourable opposition of 1719, Maraldi, the nephew of Cassini, not only confirmed his uncle's conclusions as to the rotation-period, but made a highly significant discovery of his own. At the poles of the planet, he noted for the first time two brilliant white spots, which have ever since been known as the polar caps.

In the study of Mars, the systematic work of Herschel was of paramount importance. The modern study of the planet, indeed, may be said to date from his observations, beginning in 1777. At an early stage he satisfied himself that 'the constant and determined shape' of the spots, 'as well as remarkable colour, show them to be permanent and fastened to the body of the planet'. Apparently, he discovered the polar caps independently of Maraldi. Under date 17 April 1777 he writes: 'There are two remarkable bright spots on Mars,' and in a foot-note to the paper on Mars, communicated to the Royal Society in 1784, he mentions Maraldi's observations as if they had only just come to his notice. During the next few years, Herschel discovered that the spots varied in size, and that this variation was connected with the Martian seasons. 'I may well be permitted to surmise', he wrote in 1784, 'that the bright polar spots are owing to the vivid reflection of light from frozen regions; and that the reduction of these spots is to be ascribed to their being exposed to the Sun.' His general conclusion was that 'the analogy between Mars and the Earth is perhaps by far the greatest in the Solar System'. 'The planet', he concluded, 'has a considerable but moderate atmosphere, so that its inhabitants probably enjoy a situation in many respects similar to ours.'

Herschel's study of Mars was but an incident in his wider investigations of the construction of the heavens. Apparently he did not systematically study the planet after 1784, and accordingly he made no long-continued series of drawings, nor did he attempt to chart the surface-markings. This was accomplished by Beer and Mädler, whose careful survey extended over five oppositions, from 1830 to 1839. Their telescope was one of four inches aperture, and accordingly only the more conspicuous surface-features are to be found on their map. From 1840 to 1877 the planet was observed and sketched by a number of eminent observers, including Kaiser in Holland, Secchi in Italy, Dawes, Green, Lockyer, and Proctor in England. In 1870 the last-named astronomer published a chart—based on drawings by Dawes—considerably more elaborate than that of Beer and Mädler. The red and green areas were designated as continents and oceans respectively, and Proctor named them after eminent astronomers. Thus, on his chart there appeared Herschel Continent, Dawes Ocean, Beer Sea, Mädler Continent, Cassini Land. The prevalent view of Mars in the 'seventies is well summed up in Proctor's phrase, ' the miniature of our Earth '. ' Shall we recognize in Mars ', he asked, ' all that makes our own

FIG. 11. GIOVANNI VIRGINIO SCHIAPARELLI

world so well fitted to our wants—land and water, mountain and valley, cloud and sunshine, rain and ice and snow, rivers and lakes, ocean-currents and wind-currents—without believing further in the existence, either now or in the past, or in the future, of many forms of life ? '

During the favourable opposition of 1877, Schiaparelli, working at the Brera Observatory in Milan, commenced the intensive study of Mars which rendered the study of our neighbour world almost a distinctive branch of astronomy. Giovanni Virginio Schiaparelli was perhaps the greatest ' watcher of the skies ' whom the second half of the nineteenth century produced. A man of erudite scholarship, who could have filled a chair of Oriental languages, Biblical exegesis, philosophy, or theology with distinction, he devoted his life-work to descriptive astronomy, and in the study of each of the nearer planets his work marked an epoch. His work on Mars extended over a period of about thirteen years, from 1877 to 1892. In the autumn of 1877 he commenced a trigonometrical survey of the planet, and in the course of that survey he made the discovery that the continental areas were intersected by a number of straight dark lines. These lines he designated by the Italian word ' canali ', which actually means ' channels ', but was translated into English as ' canals '. It subsequently appeared that Beer and Mädler, and later Dawes and Secchi, had seen some of the more prominent of these markings, but had simply classified them as ' straits '. It was Schiaparelli, however, whose observations revealed them as a distinctive feature of the planet. In 1879 he again noticed the canals, and in addition found that one of them had become double. At the opposition of 1882–3 he discovered the gemination, as he called the phenomenon, of several other canals. ' The observation of the gemination ', he said, ' is one of the greatest difficulty, and can only be made by an eye well practised in such work, added to a telescope of accurate construction and of great power. This explains why it was not seen before 1883.'

Schiaparelli's discoveries were made in the face of widespread scepticism. It was not generally recognized that to see the canals at all—to say nothing of their gemination—required a combination of favourable atmospheric conditions, fine instrumental equipment, and observational skill of the highest order, which Schiaparelli undoubtedly possessed. In 1886, however, Perrotin and Thollon, using the great telescope of the Nice Observatory, succeeded in seeing the canals, and soon afterwards these features were recognized by other observers, chiefly Flammarion, Stanley Williams, and W. H. Pickering. In 1892 the last-named astronomer, observing at Arequipa in Peru, carried Martian astronomy a stage farther. In his paper on 'Mars in 1892', in describing the features which he and his collaborator, Mr. Douglass, noticed, he remarked : 'Scattered over the surface of the planet, chiefly on the side opposite to the two seas, we have found a large number of minute black points. They occur almost without exception at the junction of the canals with one another and

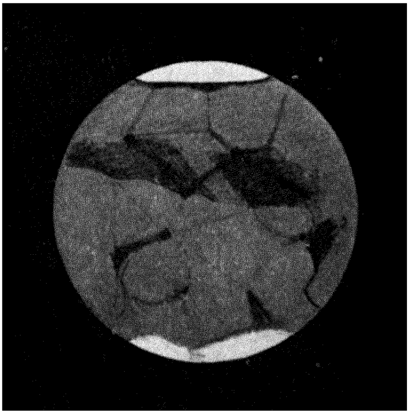

FIG. 12. MARS IN 1907

From a drawing by the late Dr. Percival Lowell with the shaded portions of the planet.' Over forty of these were enumerated. Several of them had been previously seen by Schiaparelli, who noted their connexion with the canal-system, but Pickering first detected them in considerable numbers.

For convenience, Pickering termed these objects 'lakes', in keeping with the hitherto accepted view that the shaded areas on Mars represented water. The observations of Schiaparelli, however, had thrown grave doubts upon the validity of this opinion. He noticed, for instance, the seasonal change in the colour of the seas, although in the case of the larger 'oceans', so-called, he still held in 1892 to the traditional interpretation. Other regions, which changed colour with the seasons, he was inclined to regard as 'huge swamps, in which the variation in depth of the water produces the diversity of colour'. Pickering and Douglass noticed at the same opposition 'certain curved branching lines' in the dark areas. 'Some very well-developed canals cross the oceans. If these are really water-canals and water-oceans there would seem to be some incongruity here.' 'I very much doubt', Pickering concluded, 'if what are usually known as canals and oceans contain any water at all.'

In 1894 the late Dr. Percival Lowell erected at Flagstaff, in Arizona, the well-known observatory which bears his name. Lowell realized what was previously but dimly understood, that 'a steady atmosphere is essential to the study of planetary detail, size of instrument being a very secondary matter. A large instrument in poor air will not begin to show what a smaller one in good air will; when this is recognized, as it eventually will be, it will become the fashion to put up observatories where they may see rather than be seen'. Before deciding on his site, Lowell visited France and Algeria and various American states, in order to compare the atmospheric conditions. Holding that plateaux are much more ideal localities than mountain-tops—on account of the existence of swirling air-currents around the latter—Lowell finally fixed on Flagstaff, on the great plateau of Northern Arizona, at a height of 7,310 feet. The Lowell Observatory was equipped originally with an 18-inch refractor, and a 24-inch refractor and a 40-inch reflector were later added to the equipment. The avowed object of the observatory from its foundation was the study of the planets, and especially Mars. During the opposition of 1894, Lowell carefully observed the planet from 24 May 1894 to 3 April 1895, assisted by Professor W. H. Pickering and Mr. A. E. Douglass. In the first of his books on Mars, published at the end of 1895, he set forth certain definite conclusions as to the planet's physical condition. What Schiaparelli's later observations had hinted at, and what Pickering in 1892 very cautiously surmised, Lowell was able to state with confidence. Several independent phenomena, such as the changing tints of the seas and the indisputable presence of canals therein, agreed in showing ' that the blue-green areas on Mars are not water but, generally at least, areas of vegetation '. Again, the reddish-ochre areas, which figured in Proctor's map as the continents, were described by Lowell as ' not only land but nothing but land, land very pure and simple—that is, deserts. For they behave just as deserts should behave—that is, by not behaving at all.' Lowell emphasized the vast differences between Mars and the Earth—the thin atmosphere, ' thinner at least by half than the air upon the summit of the Himalayas,' the immense tracts of desert, the absence of permanent bodies of water, and the supreme importance of the polar cap in the economy of the planet. Lowell and his co-workers, in the course of their careful study of the southern cap, detected a dark blue belt, which altered its position as the cap shrank in size. This, as Lowell clearly showed, was a temporary sea, and he demonstrated that the canal-system was dependent on the melting of the ice-fields and the formation of these seas. ' The visible development of the canal-system follows the melting of the polar snows. Not until such melting has progressed pretty far do any of the canals, it would seem, become perceptible.' Canals, seas, and lakes—or, as he chose to call them, ‘ oases ’—were viewed by Lowell as vegetal ; the canals being relatively narrow strips of country fertilized by waterways much too small to be visible to terrestrial observers. On the basis of this interpretation of the canal-system and of the known scarcity of water on the planet, Lowell founded his hypothesis of intelligent life on Mars. This theory—that the inhabitants have constructed the canal-system with the express purpose of conveying water from the melting snow-fields to the parched deserts, and that the ‘ oases ’ are, in general, centres of population—was not cordially received by the scientific world at large, and it is to be feared that the significance of Lowell's work as an observer and as a collector of data was somewhat underrated, because of unpopularity of this hypothesis.

For twenty-two years, until his lamented death in 1916, Lowell, ably assisted first by Douglass, and later on by Lampland and the brothers Slipher, followed the planet for long periods before and after opposition. In 1904 he announced that the variable visibility of the canals is not haphazard, but seasonal and dependent on latitude. He drew attention to ‘ a chronic connexion between the two phenomena, the disintegration of the cap and the integration of the canals ’. A wave of verdure begins to sweep over the planet, not from the equator polewards, but from the poles equatorwards, indicating that the floral seasons on Mars ‘ as affecting the canals are conditioned, not as they would be with us, directly upon the return of the Sun, but indirectly through its effect upon the polar snows ’. Apart from the hypothesis which he so ably championed, Dr. Lowell's conclusions as to the physical condition of the planet have been on the whole abundantly confirmed, not only by Professor W. H. Pickering in recent years, but also by Mr. E. C. Slipher, who carries on the Martian work at the Lowell Observatory, and who claims that his recent photographic work gives ‘ faithful support to the conclusions reached by Lowell concerning the physical conditions existing on Mars ’.

The conclusion that Mars, though in many respects totally unlike the Earth, belongs to the class of living worlds has been challenged by those who, on theoretical grounds, have maintained that the planet's temperature is very low, near 0° F., or considerably below freezing-point. If this is actually the case—if Mars receives only three-sevenths of the heat which falls on the Earth—then all life, not only animal but vegetable, would appear to be impossible. For a time, indeed, considerable favour was shown to the theory that the polar

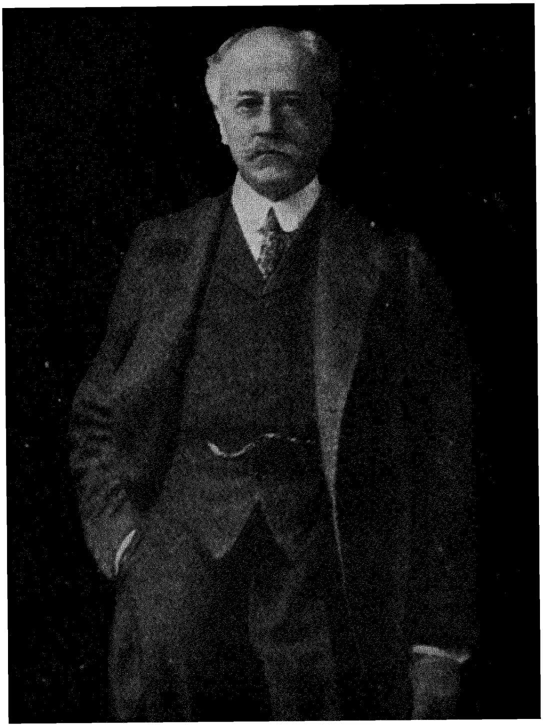

FIG. 13. PERCIVAL LOWELL

caps consisted, not of snow and ice, but of frozen carbonic acid gas, and that indeed water was non-existent in the Martian atmosphere. Some support was given to such a contention when Campbell, at the Lick Observatory, failed to confirm the earlier conclusions of Huggins and Vogel as to the presence of lines of water-vapour in the spectrum of Mars. But in 1908 Dr. V. M. Slipher detected these lines. Apart altogether from his observations, however, the appearance of the planet and the changes which take place under the eye of the observer—cloud-formation, snow-storms, melting of snow-fields, flooding of marshes—testify clearly that Mars must have a higher temperature than 0° F. 'The fundamental fact on which we must base our knowledge of the climate of Mars', as Pickering has remarked, 'is the melting of its polar snows. When these are melting the temperature must be 32° F.; and nearer the equator it must be warmer.' Lowell computed the mean temperature of the surface-air as 48° F. 'That of the Earth', he pointed out, 'is only 60° F.; so that the mean climatic warmth of the two planets is not very unlike and far within the possibilities of life for both.'

At the opposition of 1924 the problem of the Martian temperature was directly attacked by Messrs. Lampland and Coblentz at the Lowell Observatory and by Messrs. Petit and Nicholson at Mount Wilson. The investigations of the two first-named astronomers were carried out by the radiometer, an extremely sensitive instrument for measuring heat radiation, attached to the 40-inch Lowell reflector. Radiometric measurements were made of different regions of the surface, such as the polar and equatorial regions, and of the same regions at different hours of the day. The measures show that the equatorial regions, as was to be expected, are much warmer than the poles, that the afternoon temperature is higher than the morning, and that the blue-green areas are considerably warmer than the desert lands. 'The measurements', Messrs. Lampland and Coblentz conclude, 'indicate that the temperature of the brightly illuminated surface of Mars is not unlike that of a cool bright day on the Earth, with temperature ranging from 7° to 18° C. or 45° to 65° F.' The Mount Wilson results are not widely divergent ; Messrs. Petit and Nicholson found 45° F. as the temperature of the centre of the disc, that is to say, at Martian noon. These measurements have thus fully confirmed the theoretical results of Lowell and Pickering.

The question of temperature, then, would appear to be settled. We are led to inquire why Mars should be so much warmer than theoretical considerations would lead us to suppose. Coblentz finds a possible explanation in the type of vegetation prevalent upon Mars. This vegetation he believes to be predominantly of the type of tussock grasses, mosses, and lichens. Pickering suggested as long ago as 1906 that the relatively high temperature of the Martian surface is due to two main causes. 'The relatively large amount of cloud and illuminated snow areas of the Earth cause a waste of heat by reflection, which does not occur on Mars,' while the comparative cloudiness of the Martian sky at night 'would help to protect the planet from radiation of its heat into space during that portion of the Martian day, and thus also tend to raise its temperature.' And although, on the whole, Mars has a much less cloud-laden atmosphere than ours, yet on occasion cloud-masses partially conceal a whole hemisphere. The more conspicuous clouds, as Pickering has pointed out, lie along the limb and terminator of the planet. 'Since they are found both at sunset and sunrise, it is believed that cloudy nights are not infrequent upon the planet.'

The observations both of Pickering and Slipher at the oppositions of 1920, 1922, and 1924, have indicated that the Martian atmosphere is considerably cloudier than Lowell believed. 'An outstanding feature of the observations of Mars at the 1920 apparition', wrote Mr. Slipher, 'was the unusual, extensive and conspicuous cloud-like forms seen over its surface. The areas affected were particularly large and distinct.' Observations indicating a greater depth and density for the Martian atmosphere than was formerly conceded were secured at the opposition of 1924. At the Lick Observatory, Dr. Wright, photographing Mars with the Crossley reflector through screens transmitting only certain colours, concluded that the Martian atmosphere was at least 120 miles in depth and possessed appreciable absorbing and light-scattering power, while Professor Pickering stated that 'probably haze covering the entire planet occurred this last year. . . . Indeed, we often remarked ourselves that Mars seemed especially lacking in contrast at this apparition.'

Early in 1925 Dr. Adams and Dr. St. John of the Mount Wilson Observatory secured definite evidence of the existence of water-vapour in the Martian atmosphere, thus confirming the earlier work of Dr. Slipher, which had not quite convinced the sceptics. The Californian astronomers detected the presence of water-vapour to the amount of 5 per cent. of that normally present in our atmosphere. Pickering has shown that this observation was made at the driest season in the Martian southern hemisphere. This, he truly remarks, is not unfortunate, but rather the reverse, for it indicates that 'even at the driest season there is a very appreciable percentage in the Martian atmosphere'. More important still, Adams and St. John detected free oxygen to the extent of 15 per cent. of that found in our atmosphere. Accordingly, Professor Pickering believes that it is time for observers of Mars to adopt the view that 'not merely its temperature but also its atmospheric pressure closely resembles that found on Earth'. This view is confirmed by deductions which Dr. Coblentz has made from his radiometer work, which suggests the possibility of the atmosphere being more extensive than has been generally supposed.

Theories of the nature and origin of the canal-system are in the main dependent on the view taken as to temperature. Those astronomers who accepted the low theoretical temperature were generally inclined to accept the illusion theory of the canals first put forward by Cerulli of Teramo. This theory —in its original form, at least—did not survive Dr. Lowell's searching criticism, and received a heavy blow when in 1905 he succeeded in photographing some of the prominent canals. Quite evidently the canal-system is objectively real, is vegetal in its nature, and is obviously dependent, as Lowell clearly showed, on the melting of the polar snows. Here certainty ends, and we enter the realm of speculation. Lowell maintained at the close of his career that since the theory of intelligent life was first enunciated ' every new fact discovered has been found to be accordant with it. Not a single thing has been detected which it does not explain.' And Mr. E. C. Slipher, of the Lowell Observatory, in a recent pamphlet, has vigorously restated the theory. During the past decade Professor W. H. Pickering has considered a number of hypotheses in turn. In 1915 he suggested two possible variations of Lowell's theory ; firstly, that the canals consist of a growth of dark vegetation like trees and bushes on plains of a lighter colour, being supported by water artificially drawn from the natural aerial circulation of the planet ; and secondly, that fogs might be artificially localized in certain selected regions. ' It is known as a laboratory experiment that fog can be induced to form in a saturated atmosphere if furnished with a sufficient number of minute solid nuclei on which it may condense. It is suggested that it might be practical to do this either by electrifying the air in certain regions, night after night, or by some such similar means upon a large scale on Mars.' In 1918 Pickering suggested another tentative theory, according to which the function of the canals is ' to furnish by evaporation during the Martian summer a steady and continuous supply of water after that derived from the northern snows has appreciably diminished '. The water from the melting cap, according to this theory, is deposited in three main depressions in its borders, and is then evaporated and carried by aerial circulation along curved lines. ‘The storm-clouds which in that rare atmosphere should condense at night, would in this manner carry the water and deposit it in the elongated marshes which we see and call canals.’

It has long been known that some canals not only show double from time to time, but that it is not uncommon for canals to shift their positions. These phenomena, if objectively real, are not easily explained on Lowell’s original theory. As Pickering has recently pointed out, ‘it seems unlikely that a canal shifting its location through, let us say, 150 miles can owe its origin to vegetation growing along the sides of an irrigating ditch or covered aqueduct’. On the other hand, he frankly admits that, on the shower-track theory, the difficulty is not that the canals shift so much, but that they shift so little. In his latest paper Professor Pickering remarked that the finer and straighter canals can hardly be due to air-currents, and that they would seem to imply the existence of intelligent life. Pickering’s work during the last decade suggests that, if intelligence is really behind the canal system, the method adopted has been not world-wide construction of waterways, but some form of atmospheric control. Recent work on the temperature and atmosphere of the planet has added weight to the view that Mars is both a habitable and inhabited world, and that in Pickering’s words, ‘if intelligent life exists there, as the straight and narrow lines seem to imply, then the evidence adduced indicates that it need not be so very unlike ourselves as we have heretofore been led to surmise’. The spectroscopic proof of the presence of free oxygen in the Martian atmosphere is obviously of the greatest importance in this connexion. Animal life, Pickering now believes, ‘may readily exist in Mars. Indeed, a possibility exists that even human life, if transported to Mars, might exist and perhaps flourish there’. The wheel of astronomical opinion has therefore come round full circle to the view expressed by Herschel that ‘the planet’s inhabitants probably enjoy a situation in many respects similar to ours’.

Mars differs from the Earth very markedly in respect of its satellite system. In 1830 Mädler made a careful search for a possible Martian satellite, and his failure convinced him that no moon existed with a diameter greater than about twenty-three miles. D'Arrest repeated the search in 1862, but also without success. The general view, indeed, was that Mars had no attendant satellites, and this view found poetical expression in Tennyson's well-known line in reference to ' the snowy poles of moonless Mars '. Professor Asaph Hall, of Washington, was not, however, one of the sceptics. Taking advantage of the exceptionally close approach of the planet in 1877 and of the large 26-inch telescope of the Washington Observatory, Hall initiated a careful scrutiny of the planet's immediate surroundings. On 11 August his search was rewarded by the detection of a tiny point of light. By 16 August he was convinced that this was indeed a Martian attendant, and on the following evening he picked up a second tiny body still closer to Mars. These moons, which were called by their discoverer Phobos and Deimos, were soon seen to be altogether eccentric bodies. Hall found that Deimos, the more distant, revolved in 30 hours 18 minutes round its primary, from which its distance was but 12,500 miles ; while Phobos, the nearer, turned out to be only 3.760 miles[1] from Mars, and to be revolving in 7 hours 39 minutes ; that is to say, the little moon moves round Mars three times for one rotation of Mars on its axis. Thus, to a Martian observer, this tiny moon appears to rise in the west and set in the east ; while owing to its closeness to Mars, Phobos is quite invisible in latitudes about 69 degrees north or south owing to the curvature of the planet.

From photometric measures at Harvard in 1877 and 1879 E. C. Pickering estimated the Martian moons to be not more than six or seven miles in diameter. Lowell's measures in 1894 showed them to be considerably larger. He found Phobos to be about 36 miles in diameter, and Deimos 10 miles.

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[1] These distances are measured from the Martian surface.

In any case, the diminutive size of these satellites suggests that they are a different type of body from our own Moon, and that possibly they have had a different origin. The idea has been suggested that these moons were originally asteroids, which in the remote past revolved in orbits so near to that of Mars that they were captured by the planet. Certainly Phobos and Deimos bear a much closer resemblance to the asteroids than to our Moon or to the large satellites of Jupiter and Saturn.

V

THE GIANT PLANETS

THE four giant planets may be conveniently divided into two sub-classes. The first is composed of Jupiter and Saturn, by far the largest bodies in the Solar System with the single exception of the Sun itself. These two planets are prominent objects in our night skies. Saturn is equal in brilliance to a star of the first magnitude, while Jupiter is much more brilliant. Accordingly these two worlds have been known from prehistoric times. The second group comprises Uranus and Neptune, which, though undoubtedly falling into the category of giant planets, are considerably smaller, and because of this, and also by reason of their much greater distance, were both quite unknown less than a hundred and fifty years ago.

The study of Jupiter dates from the time of Galileo, whose most striking telescopic discovery was that of the four large satellites of the giant planet in January 1610. His telescope was not powerful enough, however, to reveal any markings on the Jovian disc, and it was not till 1630 that Zucchi, an Italian observer otherwise unknown to fame, first noticed the parallel lines since known as the belts of Jupiter. Thirty-four years later a still more significant discovery was made, when the English astronomer, Hooke, detected a small spot which he perceived to be in motion. His suspicion that this indicated a rotation of Jupiter on its axis was confirmed in

1665 by Cassini, who determined the period as 9 hours 55 minutes, which, like most of Cassini's determinations, was a remarkable approximation to the truth.

The first interpretation of the Jovian markings was put forward by Sir William Herschel in 1781. In a paper on the rotation of the planets on their axes, he threw out the hint that the belts represented currents in Jupiter's atmosphere, somewhat analogous to the terrestrial trade-winds. This view was in harmony with Herschel's general conception of planetary conditions. To him it was an axiom that our neighbour worlds were ' richly stored with inhabitants ', and thus he was led to interpret his observations of Jupiter in the light of the hypothesis that the great planet was simply an immensely larger edition of the Earth. During the greater part of the nineteenth century this view of Jupiter prevailed, and was accepted by most of the popular writers on astronomy. J. P. Nichol, the Scottish astronomer, for instance, interpreted the spots as mountains, in keeping with the current view, which was held also by Sir John Herschel.

There had been, it is true, doubts as to the validity of the parallel drawn between Jupiter and the Earth. Buffon in 1778 and Kant in 1785—neither of them astronomers—had suggested that Jupiter was still in a state of great internal heat, and the same suggestion was made by Nasmyth in 1853. But it was not until 1865 that the evidence adduced by Zöllner of Leipzig convinced contemporary astronomers that Jupiter was no enlarged edition of our own world, but a veritable semi-sun. Zöllner drew attention to the rapid changes in the cloud-belts both of Jupiter and Saturn. Such changes, he showed, indicated clearly a high internal temperature for both planets, for the heat of the Sun at the distance of Jupiter is quite incapable of causing atmospheric disturbances on so vast a scale. Zöllner drew attention, too, to another analogy between Jupiter and the Sun, namely, that the planet, as Herschel had noted long before, does not rotate as a whole, but that the rotation is accelerated in the equatorial regions. Clearly, this fact conflicted directly with the current view of the planet's physical condition. The new view of Jupiter and of the giant planets in general was adopted by Proctor early in his career, and presented with convincing logic in 1870 in his *Other Worlds than Ours.*

> 'If analogy is to be our guide,' he wrote, 'and we are to judge of the condition of Jupiter according to what we know or guess of the past condition of the Earth and the present condition of the Sun, we seem led to the conclusion that Jupiter is still a glowing mass, fluid probably throughout, still bubbling and seething with the intensity of the primeval fires, sending up continually enormous masses of cloud to be gathered into bands under the influence of the swift rotation of the giant planet.'

The conception of Jupiter as a body half-sun, half-world has met with practically universal acceptance.

In the seventies, a new period in the study of Jupiter was inaugurated, which may be called the period of intensive observation. The discovery by Niesten at Brussels in July 1878 of the remarkable object known as the 'great red spot' gave an impetus to much more detailed study of the spots—both bright and dark—and the more delicate markings on the Jovian disc. Eminent observers, such as Keeler, Lowell, and Barnard, kept the planet under observation with the aid of the most powerful telescopes; and in addition, a great deal of important information was collected by several non-professional astronomers, among whom must be specially mentioned Messrs. Denning, Stanley Williams, Molesworth, and Bolton. Denning's patient study of the great red spot has shown that it was probably visible long before 1878; he believes it to have been seen by Schwabe in 1831, and probably by Hooke in 1664. The long-continued work of Mr. Stanley Williams indicated to that astronomer in 1896 the existence of nine different currents in the Jovian atmosphere, made manifest by the different rotation-periods obtained from observations of spots in the various latitudes. In 1901, Molesworth, observing in Ceylon, was the first to catch a glimpse of the beginnings of the 'southern tropical

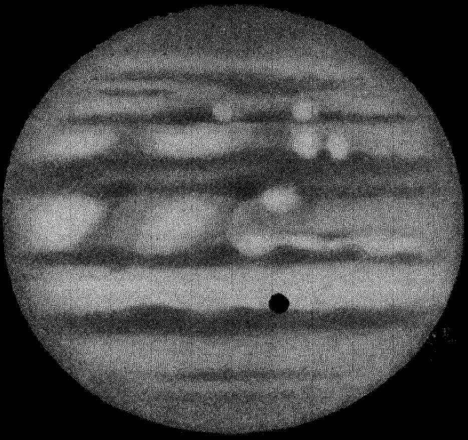

FIG. 14. JUPITER, 11 JUNE 1925
Shadow of third satellite (Ganymede) on disc

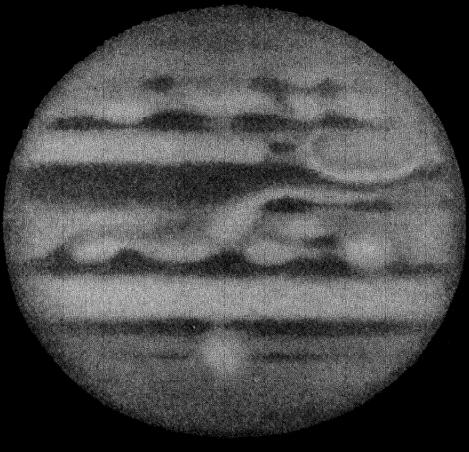

FIG. 15. JUPITER, 16 AUGUST 1925
The 'Great Red Spot' and its 'hollow' are on right

disturbance ', which has been conspicuous on the disc for the past twenty years. Another significant discovery, due to Mr. Scriven Bolton, was confirmed by Professor Lowell at the Lowell Observatory. Mr. Bolton detected ' wisps ' or ' lacings ' extending across the bright equatorial belt, and Lowell pointed out that these wisps are not confined to the equatorial belt, ' but are to be seen traversing all of the bright belts both north and south up to the polar hoods '.

Several attempts have been made in recent years to correlate in a general theory of the planet's constitution all the isolated facts which have been ascertained during the past half-century. The late Danish astronomer, H. E. Lau of Copenhagen, put forward shortly before his premature death a theory of the planet based on his own observations. The true surface of the planet, he maintained, consists of a bright yellow cloud envelope, highly reflective, enveloped in a much thinner atmospheric stratum. The equatorial zones he believed to be spot-zones, the belts consisting of dark spots. ' The bright spots are apparently clouds overlying the hottest places. Their brightness varies with the sun-spot activity, so that a periodic change of brightness in the planet is produced. The red spot is an abnormally heated region in the deeper strata of the gaseous envelope.' This does not conflict with Lowell's view that the red spot is due to a ' vast uprush of heated vapour from the interior . . . a sort of baby elephant of a volcano, or geyser, occurring as befits its youth in fluid, not solid conditions, but fairly permanent nevertheless—a bit of kindergarten Jovian geology '. The colour of the spot was thought by Lowell to indicate that ' in such places we look down into the fiery, chaotic turmoil incessantly going on '—a view held also by Denning. Some authorities maintain, however, that nothing but cloud is visible—that we cannot even glimpse the true Jovian surface; on the other hand, Barnard has recently expressed the view that the surface we see is not ' exactly a cloud surface '. ' The appearance is more that of a pasty nature.'

Recent research has undoubtedly emphasized the resemblance between Jupiter and the Sun. Lau, in his theory of Jupiter, suggested that the rapid rotation of the planet is the explanation of the existence of continuous belts in place of isolated spot-groups, such as are seen on the slowly rotating Sun. That the red spot is akin to sun-spots has been recognized for many years. As long ago as 1879 the Russian astronomer Brédikhine noticed a group of objects resembling the solar faculae encircling the spot. Phillips inclines to the view that ' the red spot is a vortex analogous to a cyclone on the Earth, though its prolonged existence shows that it must be of great strength and probably deep-seated below the planet's visible surface '. ' We might indeed expect many vortices to occur along the lines of contact between viscous currents possessing diverse velocities, and if many of the Jovian spots are of this character we have another analogy between Jupiter and the Sun.' Amid considerable diversity of opinion regarding the finer detail on the disc of Jupiter, it may be said that the work of the last two generations has abundantly confirmed the view that Jupiter, in common with the other giant planets, is in a state of primeval chaos. As Barnard remarked, ' They are the worlds of the future. Perhaps millions of years hence they will be discussing the possibility of life on the Earth—if they ever know of its existence—or whether life ever existed there.'

In 1923, however, Dr. Harold Jeffreys, the well-known English mathematician, put forward an alternative theory. The data, he maintained, are more easily reconcilable with the hypothesis that Jupiter and the other giant planets ' are cold and solid, their material being of low density in comparison with terrestrial rocks '. The atmospheres are very extensive, the depth being one-fifth of the radii. These atmospheres, Dr. Jeffreys thinks, must be cold, and the suspended clouds ' are probably composed of some material with much lower boiling and melting points than water '. The theory is based on Dr. Jeffreys' speculations concerning the evolution of the planets, and is deductive rather than inductive. Superficially, some support would seem to be lent to it by the radiometric work of Messrs. Coblentz and Lampland at the Lowell Observatory, which indicate no appreciable planetary radiation, but this is counterbalanced by the work of Mr. Menzel, the Harvard observer, who finds real traces of internal heat.[1] Coblentz and Lampland certainly do not interpret their results as in keeping with any such hypothesis as Dr. Jeffreys', and they hold that 'in the present state of our knowledge we cannot conclude that Jupiter does not actually send out considerable amount of planetary radiation'. The observations on Saturn made by the same observers, which indicate a high temperature, certainly do not confirm Dr. Jeffreys' theory.

All that is known, indeed, of Saturn abundantly confirms this view. The belts of Saturn are fainter and less conspicuous than those of Jupiter solely because of the much greater distance at which that planet revolves. Doubtless the relative scarcity of spots is due to the same cause. It was not until 1794 that the appearance of a Saturnian spot gave the clue to the period of rotation. In that year Herschel perceived one, and from its motion fixed the length of the planet's 'day' at 10 hours 16 minutes. This period was more or less confirmed by Hall at Washington in 1876, and later by Denning and Stanley Williams. A bright spot noticed by Barnard at the Yerkes Observatory in 1903 indicated a considerably longer period—10 hours 39 minutes, a fact which demonstrates that Saturn, like Jupiter, does not rotate as a whole.

Saturn resembles Jupiter not only in the broad general features of its disc, but in the more intricate detail as well. Highly significant was the discovery by Lowell of 'wisps' similar to the wisps of Jupiter—'filamentous streaks between the belts, sometimes vertical but more often inclined, after the manner of the lacings of a sail. Individually they resemble distant droppings of rain from a storm cloud seen slanting against the sky under the force of a strong wind. At first they were described only across the light equatorial zone, but have since been seen meshing the whole surface of the disc.'

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[1] Menzel, Coblentz, and Lampland (*Astro-physical Journal*, LXIII, pp. 183–4) regard their more recent work on the giant planets as confirmatory of the conclusions reached by Jeffreys.

The two giant planets differ from the dwarfs in the possession of important and genuine systems of satellites. The Earth-Moon and the Martian systems both appear to be somewhat sporadic cases, out of the regular course of evolution. The genesis of the Moon, as Sir G. H. Darwin's researches indicated, was possibly unique ; while, as already mentioned, the satellites of Mars are bodies so closely resembling the asteroids that it is by no means improbable that Mars was originally moonless, and that the little attendants were asteroids which came too near to the red planet at some epoch in the distant past. But the satellite-systems of the outer planets are of a different order. The four large satellites of Jupiter, discovered by Galileo in January 1610, are in point of size only slightly inferior to the inner planets, and only their great distance from us renders them more or less mysterious objects. The largest telescopes in the world have been employed on these satellites from time to time, but authentic information concerning them is scanty. Polar caps, dusky markings, and canaliform appearances have been glimpsed, but the evidence is conflicting. More certain is the likelihood, which Herschel, with his quick insight, grasped over a century ago, that each of the four moons completes its rotation in the same period as its revolution, and turns the same face constantly to its primary, as the Moon does to the Earth and Mercury does to the Sun. But in the case of the third satellite, Ganymede, this conclusion is contested by Professor W. H. Pickering.

Probably no discovery in recent years was so totally unlooked for as that of the tiny fifth satellite of Jupiter by Barnard at the Lick Observatory in September 1892. For nearly three hundred years the Jovian system had been regarded as consisting of the four large satellites—Io, Europa, Ganymede, and Callisto. The tiny fifth moon, no more than

100 miles in diameter, whirling round its primary in a period of 11 hours 57 minutes at a distance of 112,000 miles, seemed strangely out of place among the larger satellites, and the suggestion was hazarded by Sir Robert Ball that it was but the first of a number of small satellites. Other four minute moons have since been detected—two by Perrine at the Lick Observatory in 1905, one by Mellotte at Greenwich in 1908, and another by Nicholson at the Lick Observatory in 1914. These objects, so faint as to be detected only by their images on the photographic plate, are akin to the fifth satellite in size, but not in distance; for the sixth and seventh moons are about seven million miles from Jupiter and require 250 and 260 days respectively for their revolution; while the eighth and ninth, which revolve in a retrograde direction, are about eighteen million miles away and require over two years for their circuit of their primary. The idea is at once suggested that these tiny Jovian satellites are captured asteroids, and that the four large moons are the only original members of the system. Be this as it may, there can be no doubt that the satellites of Jupiter fall into two well-defined groups, giant and dwarf moons.

The same cannot be said of the Saturnian system. The nine or ten satellites of which that system is composed are of various degrees of brilliance and presumably of different sizes. Titan, the largest, is of the same order of size as the Jovian moons. It was discovered by Huyghens in 1655. Of the other satellites, four were discovered by Cassini between the years 1671 and 1684—the brightest of these being Japetus, the second largest member of the system. In 1789 the two innermost satellites, Mimas and Enceladus, were detected by Herschel with his great 40-foot reflector. In 1848, Bond, the American astronomer, and Lassell, the English observer, independently discovered Hyperion. By the middle of the nineteenth century, eight Saturnian moons were known to exist. Considerable surprise was occasioned, therefore, by the announcement by Professor W. H. Pickering of the discovery of a ninth satellite in 1898. Ten years earlier, Pickering had commenced at Harvard a photographic search for new Saturnian moons. The search was unsuccessful, but was resumed at the Harvard southern station at Arequipa, in Peru, and this time it was successful. The very faint moving object which Pickering found on three photographs of the vicinity of Saturn was announced as a new satellite, to which Pickering gave the name of Phoebe. However, as the satellite was not again photographed and had not been observed visually, there was considerable scepticism as to the reality of the discovery. Even Pickering himself began to wonder 'if the four images discovered on the plates of 1898 might not after all have been defects or faint stars, recurring, by a curious coincidence, in exactly the proper places to represent the motion of a satellite'. On plates taken from 1900 to 1902, however, the new satellite was clearly visible, and in 1904 Pickering was able to announce that Phoebe revolved round Saturn at a distance of about eight million miles in a retrograde direction, in a period of about 546 days. In 1905 Pickering announced the photographic discovery of a tenth satellite, situated at a mean distance nearly similar to that of Hyperion; that is to say, considerably nearer than Japetus or Phoebe. This discovery has not been placed beyond doubt, but judging from the vindication of Pickering's discovery of Phoebe, it would be unwise to assume that the tenth Saturnian moon does not exist.

Saturn is unique among the planets in the possession of a system of rings. This ring-system was one of the earliest telescopic discoveries. Galileo's telescopes were powerful enough to reveal something abnormal about Saturn's appearance, but not powerful enough to show the actual cause of this appearance. Saturn appeared to Galileo as a triple star, and when in 1612 the two smaller stars disappeared Galileo's amazement knew no bounds. 'The shortness of time, the unexampled occurrence, the weakness of my intellect, the terror of being mistaken, have greatly confounded me.'

Later, he noted the return of what he took to be the two attendant stars, but he never fathomed the mystery. This was reserved for Huyghens, who in 1656, with more powerful instruments than Galileo, announced that 'the planet is surrounded by a slender flat ring inclined to the ecliptic and nowhere touching the body of the planet'. The phenomena that so puzzled Galileo now found their explanation, the vanishing of the protuberances on either side of the planet being due to the periodic disappearance of the rings when their plane happens to lie in our line of sight.

Cassini in 1675 detected a division in the ring, and this discovery proved that the planet possessed not one ring but two. In the early nineteenth century Encke discovered a division in the outer ring much less clearly marked than the main division, which has been interpreted as a thinning out rather than an empty space. Encke's assistant, Galle, in 1838, made a still more significant discovery. He discerned a kind of veil-like extension of the inner ring, over half of the space separating it from the planet. But Galle, though he announced his observation to the Berlin Academy, did not realize its significance, and it was not until 1850 that Bond, in America, and Dawes, in England, independently recognized this appearance as indicating the presence of a third ring. This, called the dusky ring or crape ring, is the innermost of the three appendages.

The earlier astronomers proceeded on the assumption that the rings were thin, solid planes, and it was not until the middle of the nineteenth century that the mathematical analysis of Roche of Montpelier, and later of Clerk-Maxwell, demonstrated that they were neither solid nor liquid, but composed of myriads of meteorites or 'brickbats', as the Scottish physicist called them, each particle revolving round Saturn in its own orbit, yet so closely packed that the rings appear at our distance as solid structures. The dusky ring found its explanation on this theory as due to the particles being more sparsely scattered.

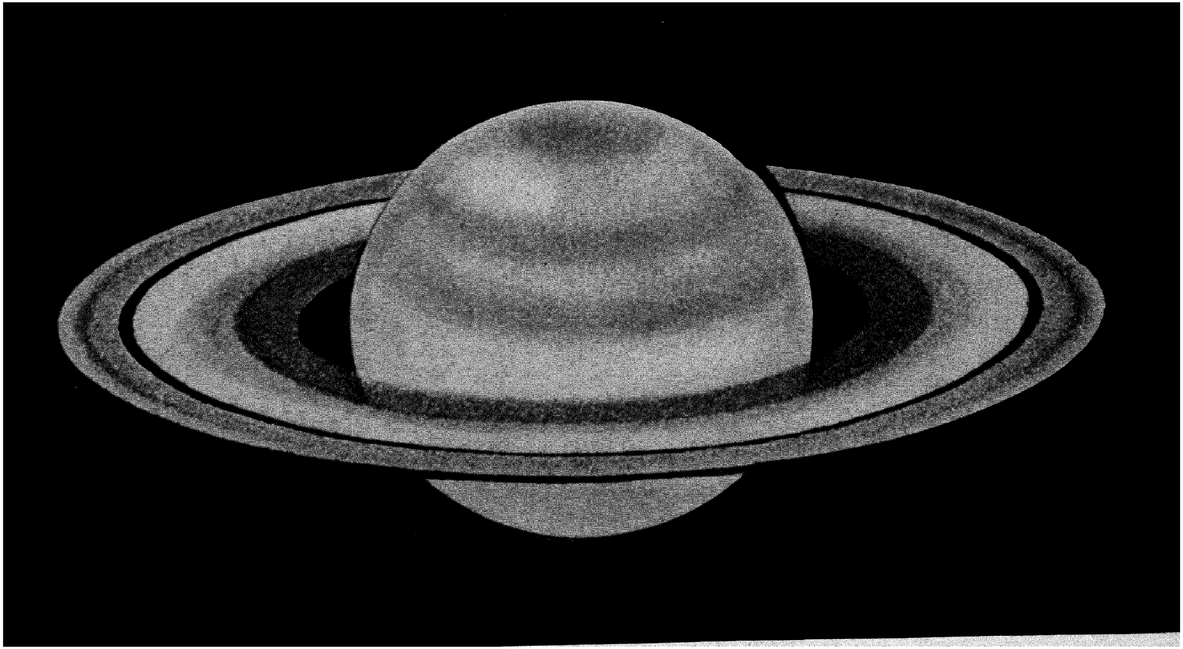

Fig. 16. SATURN, 5 DECEMBER 1910

In 1888 Seeliger pointed out that the unvarying brilliance of the outer rings under all angles of illumination was a clear confirmation of the meteoritic theory of the rings. Definite observational proof came in 1895, when Keeler, the American astronomer, applied Doppler's principle to the measurement of the motions of the inner and outer rings. Keeler found the rate of motion for the inner edge of the inner bright ring to be 12½ miles per second, and the rate for the outer edge of the outer ring to be 10 miles per second. This was positive proof that the ring-system was not a solid whole, but composed of independent particles moving in their own orbits.

Lowell commenced, early in the present century, an exhaustive study of the ring-system. His scrutiny was rewarded when in 1907 he made the highly important discovery of the knots or beads on the rings, obviously particles pulled out of the plane of the rings by some external influence. Lowell's later mathematical analysis showed that the gaps in the ring and knots on the rings were due to the pull of the several satellites. ' We find ', he wrote in 1910, ' that Mimas, Enceladus, and Tethys have periods exactly commensurate with the divisions of the rings ; in other words, these three inner satellites, whose action because of proximity is the greatest, have fashioned the rings into the three parts we know.'

Obviously the ring-system represents a satellite spoiled in the making, and evidently the rings are analogous to the asteroidal zone. In the case of the asteroids, the proximity of the huge mass of Jupiter is believed to have prevented the formation of a planet of any considerable size. In the case of the Saturnian rings, no satellite could be formed at so small a distance ; and thus we have what is to all intents and purposes a meteor stream, myriads of minute stones and particles closely packed together which at the enormous distance of nearly eight hundred millions of miles from the terrestrial observer appear as an annulus of ' immaterial light '.

The two outermost planets are conveniently grouped together for two reasons: firstly, because of their close family resemblance; they are, indeed, almost identical in size. More important still, from the point of view of the history of astronomy, they are inseparably linked together inasmuch as the discovery of the one led in time to the discovery of the other.

Uranus swam into human ken on the evening of 13 March 1781, when the keen eye of Herschel detected a strange object among the stars of the constellation Gemini. At first he believed it to be a comet, and the discovery of a comet was announced to the observatories of Greenwich and Oxford. The Astronomer Royal, Maskelyne, having observed the strange object, pronounced it ‘ very different from any comet I ever had any description of or saw ’. On 23 April he wrote to Herschel: ‘ It is as likely to be a regular planet moving in an orbit nearly circular round the Sun as a comet moving in a very eccentric ellipsis ’; and he immediately notified the French astronomers of the discovery. Messier, the most famous observer of comets, commenced observations on 16 April, and his example was followed by Lalande, Lemonnier, and other astronomers in France, and by Bode in Germany. Efforts were made to calculate its orbit, on the assumption that it actually was a cometary body. Orbits were calculated by Méchain, De Saron, Laplace, and others. These efforts were fruitless. On 8 May, De Saron announced that the ‘ comet ’ was much more distant from the Sun than had been supposed. Laplace independently reached a similar conclusion. Meanwhile, Lexell, the St. Petersburg mathematician, who happened to be in England when the discovery was made, informed the St. Petersburg Academy that the object discovered by the German musician was probably not a comet at all, but an exterior planet, revolving at twice the distance of Saturn, thus confirming Maskelyne's sagacious surmise.

Soon after the discovery, the orbit of the new planet was calculated. Bouvard, the famous French mathematician, who co-operated with Laplace, published tables of the planet, based not only on observations made since 1781, but also on observations made by astronomers who had mistakenly considered Uranus to be a star. It soon became apparent that there was a discrepancy between theory and observation. Uranus did not move according to Bouvard's predictions. Accordingly, Bouvard rejected the earlier observations altogether, and based his new tables on scantier but more reliable data. Again, however, his predictions were falsified. The irregular motion continued. Certainly the discrepancy between theory and observation was small, judged by everyday standards. If the real and theoretical Uranus had been placed side by side in the sky, they would have seemed a single planet, even to the sharpest vision.

Bouvard threw out the hint that the irregularities were due to an unseen perturbing body, and the conviction grew on the scientific world that this was indeed the case. Bessel, of Königsberg, one of the first mathematicians of the day, formed the design of finding out from the observed perturbations the position of the unseen planet in the sky ; but before he was able to attack the problem he was stricken by fatal illness. Meanwhile, in 1841, a Cambridge undergraduate noted in his diary his resolve to investigate ' the irregularities in the motion of Uranus, which are as yet unaccounted for, in order to find whether they may be attributed to the action of an undiscovered planet beyond it : and, if possible, thence to determine the elements of its orbit approximately which would lead probably to its discovery '. After taking his degree in 1843, J. C. Adams commenced his laborious task. After two years' steady work, he came to the conclusion that a planet revolving at a certain distance beyond Uranus would account for the observed irregularities, and he handed to Challis, the director of the Cambridge Observatory, the elements of what he called the ' new planet '. On 21 October 1845, he called at Greenwich Observatory and left for the Astronomer Royal a paper containing the elements of the unseen planet and a determination of its place in the sky.

The Astronomer Royal was a busy man, and apparently was not impressed. He wrote to Adams, however, asking a question which he felt to be of considerable importance. Adams, either by reason of negligence or pique, failed to reply, and so Airy took no steps to search for a planet in which he did not really believe. An unfortunate accident, moreover, prevented the discovery being made elsewhere as a result of Adams' work. Dawes, the celebrated English observer, saw Adams' papers at Greenwich, and was so impressed with them that he wrote to his friend Lassell, then in possession of probably the finest reflector in England, asking him to search in the portion of the sky indicated by Adams' calculations. Unfortunately, Lassell was suffering from a sprained ankle, and his friend's letter was accidentally destroyed by a careless housemaid.

Meanwhile, the perturbation of Uranus had been attracting the attention of the French astronomers. Arago, the versatile director of the Paris Observatory, convinced of the existence of an exterior planet, urged on his young mathematical friend, Le Verrier, the advisability of an attack on the problem. Ignorant of the fact that Adams had already solved it, Le Verrier set about his task in 1845. In three memoirs communicated to the French Academy, he demonstrated, firstly, that no known causes could produce the perturbations of Uranus; secondly, that an exterior planet alone could produce them; and thirdly, that the exterior planet was situated at a certain point in the constellation Aquarius. One of these papers happened to come under the notice of Airy. Noticing that Le Verrier had reached a conclusion similar to Adams, he became at last convinced of the urgency of the question, and wrote to Challis of Cambridge, suggesting that a search should be made in the constellation Aquarius. In July 1846, Challis began the search. The planet was actually seen on the 4th and 12th of August, but was not recognized. 'After four days of observation', Challis wrote to Airy, 'the planet was in my grasp if I had only examined or mapped the observations.' Airy had been too late. In the meantime, Le Verrier had not been idle. On 18 September he wrote to Galle, then chief assistant to Encke at the Berlin Observatory, requesting him to search for the planet in the constellation Aquarius. On receipt of the letter, on 23 September, Galle consulted Encke, who directed that a search be made, and on that very evening, in the precise spot indicated by Le Verrier, the planet was found. On 29 September, Challis picked up the planet at Cambridge, but the priority of the discovery was lost to Adams. At Le Verrier's suggestion, the newly-discovered member of the Solar System received the name of Neptune.

As was to be expected, the planet Uranus was an object of special interest to its discoverer. In 1782 Herschel announced the results of a preliminary study of the new planet; he succeeded in making a remarkably accurate measure of its diameter, which he found to be 34,217 miles. Soon after the discovery of the planet, he commenced a systematic search for satellites, which was rewarded on 11 January 1787 by the discovery of two faint, star-like objects, whose motion revealed them in their true nature as satellites. In 1797 Herschel announced that he had seen other four faint objects which he suspected to be still fainter moons. The discovery was never confirmed, and the satellites were never seen again, though it is quite possible that two of them were the very faint objects seen in 1847 by Lassell and Otto Struve, and finally verified by the former astronomer four years later. The same keen-sighted observer discovered on 10 October 1846 the only satellite of Neptune with which we are acquainted. This satellite, for which the name Triton has been suggested, would appear to be one of the largest moons in the Solar System. Most likely it is one among several others, which are invisible on account of their great distance.

Very slow has been the rate of advance in our knowledge of the two outermost planets. Dusky bands resembling those of Jupiter were discerned on the sea-green disc of Uranus by Young at Princeton in 1883. In the following year the brothers Henry, two eminent French astronomers, noticed two grey parallel lines, an observation which was confirmed by Perrotin with the large refractor of the Nice Observatory. Perrotin thought also that he had evidence that the planet had a rotation period of eight hours; and a short rotation period was further hinted at by the measurements of Schiaparelli, Perrotin, and Young, which showed Uranus to be much compressed at the poles. In 1912 the length of the Uranian day was determined at the Lowell Observatory by Dr. Lowell and his assistant, Dr. V. M. Slipher. The problem was attacked spectroscopically by means of Doppler's principle, and the period was measured as 10 hours 45 minutes in a retrograde direction.

On account of its still greater distance from the Earth, Neptune guards its secrets even more jealously than Uranus. The disc of the planet is still smaller than that of Uranus. On this disc, Dr. T. J. J. See discerned on 10 October 1899 some faint indistinct belts similar to those of Uranus. In 1883 Mr. Maxwell Hall in Jamaica noted certain light-variations which suggested a rotation period of eight hours, but these suspicions were not confirmed, and it can be safely said that of the length of Neptune's day we know nothing at all. In all likelihood, however, when it is measured, it will be found to be between eight and twelve hours. The scanty knowledge which we do possess concerning the physical condition of the two outermost worlds of our system is due to the spectroscope. In 1902 Dr. Slipher began his investigation of the spectra of the four giant planets at Flagstaff. In the spectrum of Neptune he detected a hydrogen line, indicating the existence of free hydrogen in the planet's atmosphere. This again suggests a temperature sufficient to resolve water into its constituent elements; while in the case of Uranus a helium line was strongly suspected. But Dr. Slipher holds that judgement must be suspended meanwhile as to the true nature of these lines. The atmospheres of the two outermost planets would appear to be very extensive. In the case of Uranus, Lowell reached the conclusion that in that planet ' we see a body in an early amorphous state, before the solid, the liquid, and the gaseous conditions of matter have become differentiate and settled each into distinctive place. Without even an embryo core, its substance passes from viscosity to cloud.'

So far as we know at present, Neptune's orbit marks the frontier of the Solar System. From time to time during the past half-century various astronomers have attempted to repeat the feat of Le Verrier and Adams. True, Neptune's movements have not shown indications of unexplained perturbations; but there are certain ' residual perturbations, of Uranus, and in addition there is the existence of comet families attending the giant planet, and there are several comets whose orbits go out far beyond Neptune. Accordingly it has been maintained by several distinguished astronomers that there is at least one trans-Neptunian giant planet still undiscovered. Flammarion computed the possible orbit of the hypothetical body, and Lowell and W. H. Pickering assigned its elements and even its possible place in the sky. In 1910 a photographic search was made in the region indicated by Pickering's calculations, but it proved fruitless. At present, therefore, the balance of evidence is on the side of the view that the data on which these distinguished astronomers worked was too slender, and that Neptune is the most distant member of the Sun's family.

VI

ASTEROIDS, COMETS, AND METEORS

THE matter comprising the Solar System, exclusive of the Sun, may be conveniently grouped into planetary and extra-planetary. The planetary matter, as we have seen, is distributed into the two groups of primary planets—giants and dwarfs—and their satellite-systems. The extra-planetary matter consists of asteroids, comets, and meteors, along with which may be mentioned the finely divided matter which gives rise to the phenomenon of the Zodiacal Light, and the Gegenschein, or counterglow.

It has been a prevalent practice to classify the asteroids as planets, and in some of the text-books of astronomy published between sixty and thirty years ago they are to be found tabulated in appendices as primary members of the Solar System. It is not to be wondered at that they were originally so regarded, for they revolve round the Sun in orbits which, though more eccentric and more highly inclined to the ecliptic—the plane of the Earth's orbit—than the orbits of the large planets, are yet of planetary form. Nevertheless, it ought to be said that even when only two of these were known to astronomers, Herschel pointed out that they were essentially different from the planetary bodies. For this reason, indeed, he proposed to call them asteroids, a name which is now in general use.

Nevertheless, the first asteroids were discovered as the sequence to a search for a missing planet. As far back as Kepler's time it was noticed that there was a big gap in the Solar System between the orbits of Mars and Jupiter; and indeed Kepler went so far as to say that a planet might one day be discovered there. The subject first attracted widespread notice, however, when Johann Elert Bode, afterwards director of the Berlin Observatory, drew attention to a curious numerical relationship—since known as Bode's law, though pointed out before his time by Titius of Wittenberg—between the distances of the planets. If the number four be added to each of the numbers 0, 3, 6, 12, 24, 48, 96, and 192, the resulting series represents pretty approximately the distances of the planets from the Sun, thus—4 (Mercury), 7 (Venus), 10 (Earth), 16 (Mars), 28, 52 (Jupiter), and 100 (Saturn). After the discovery of Uranus it was found that it filled up the number 196; and so the fact that the number 28 had no planet to represent it seemed the more inexplicable. On the strength of this gap in the series, Bode decided that a planet existed to fill it, and in conjunction with Von Zach, of Gotha, took steps to ensure its discovery. Bode and Von Zach summoned a congress of astronomers, which met in 1800 at Schröter's Observatory at Lilienthal, and this congress decided that twenty-four astronomers should take part in the search, and to each of them was assigned a zone. One was assigned to Piazzi of Palermo, the Italian astronomer, who was not present at the congress.

While still unaware that he was expected to take part in the search, Piazzi came upon the missing planet. On 1 January 1801, the first night of the nineteenth century, while engaged on the observations for his important star-catalogue, he noticed a strange object which on the next two evenings had shifted its position. He decided that he had detected a tailless comet. He hastened to inform Bode of his discovery, and the Berlin astronomer at once concluded that the object of his special search had been found. By this time, however, the body was lost in the Sun's rays, and there were grave fears lest it would not be seen again, for an orbit had never been calculated before from observational material so scanty. At this point, Carl Friedrich Gauss, the celebrated Göttingen mathematician, then a young and struggling tutor, solved the problem by the system of least squares, his own mathematical device, and assigned to the missing object a place in the constellation Virgo. There, on 31 December 1801, it was seen by Von Zach at Gotha, and the following evening by Olbers at Bremen. The minute body thus definitely discovered was named Ceres, by Piazzi's special request.

The gap had therefore been filled. True, the new planet was disproportionately, even ridiculously, small, invisible indeed to the unaided eye. But before astronomers had time to speculate on its nature or origin, general surprise was created by the announcement by Olbers that on 28 March 1802 he had discovered a second object of the same type. Gauss demonstrated beyond a doubt that the second body, which its discoverer named Pallas, was moving in an orbit almost similar to that of Ceres, at practically the same distance from the Sun. This totally unexpected discovery gave rise to the idea that there might be other objects of the same kind yet to be found—an idea justified by the discovery of a third, Juno, by Harding at Lilienthal on 2 September 1804, and of a fourth, Vesta, by Olbers, after three years' careful search, on 29 March 1807.

Nearly forty years elapsed before further members of the group were detected. Indeed, the four seemed complete in themselves, and the general idea was that there were no more to be discovered. This was not the view, however, of Karl Ludwig Hencke, ex-postmaster of Driessen, in Prussia. In 1830 this amateur commenced a systematic search for further members of the asteroid group, and for fifteen years carried on an apparently hopeless quest. At last, on 8 December 1845, his patience and perseverance were rewarded by the discovery of a fifth asteroid, which received the name of Astraea, and on 1 July 1847 of a sixth, afterwards named Hebe. A few weeks later, J. R. Hind, an English astronomer, discovered other two. The success of Hencke and Hind inspired other astronomers to join in the search, and by the middle of the century thirteen in all were known. After 1847 the discovery of asteroids became an everyday occurrence, and not a year has passed without the addition of one or more, sometimes as many as twenty being found in a single year.

Certain astronomers, such as Peters and Luther in Germany, Watson in America, and Chacornac, the brothers Henry and Perrotin in France, devoted themselves in some cases exclusively to the hunt for asteroids, so that indeed the mathematicians by whom orbits were computed could barely keep pace with the discoveries made.

In 1891 Dr. Max Wolf of Heidelberg opened a new era in this branch of research by the application of photography to the search for asteroids. It occurred to Wolf that if a plate, fitted into a telescope going by clockwork, were exposed to any given region of the Zodiac, an asteroid, by reason of its proper motion, would be represented by a trail instead of by a disc. On 22 December 1891 Dr. Wolf found his first asteroid by this method, and in the following year the same skilful observer discovered fifteen more. Since then the discovery of asteroids by this method has gone on apace, and at the present time over a thousand are known. Apparently there is no limit to their numbers, and M. Stroobant of Brussels calculates that there are at least fifty thousand. Only the four large members of the group have been studied as individuals. These four seem to stand almost in a class by themselves. They were exhaustively studied in 1895 by Barnard, using the great 36-inch refractor of the Lick Observatory. His measures of their diameters showed Ceres to be the largest, with a diameter of 477 miles, Pallas second, with a diameter of 304 miles, and Vesta and Juno third and fourth respectively, with diameters of 239 and 120 miles. He saw not a trace of atmosphere round either of the four, which would indicate that they are simply arid rocky masses. These four large asteroids were measured by means of the micrometer ; measures of the smaller members of the group have been made photometrically, and within recent years some of the fainter members of the zone, which have been discovered in shoals since the century opened, have been measured by this method. Eros, the asteroid detected in 1898 by Witt of Berlin, which attracted much attention because of the eccentricity of its orbit, seems to have a diameter of about thirty miles. A tiny asteroid found by Wolf in 1918 does not exceed four miles in diameter ; a still smaller one was measured at Mount Wilson by Shapley and Nicholson. Its diameter came out at three miles, and it is therefore ‘ the smallest planetary object for which dimensions have yet been estimated ’. As instrumental power increases, discoveries of still smaller objects may be expected. There seems to be no lower limit to the size of asteroids, and thus there would appear to be no hard-and-fast dividing line

FIG. 17. MAX WOLF

between small asteroids and large meteors, a fact which clearly shows that asteroids fall into the category of meteoric rather than planetary bodies.

Further support has been given to this view by a study of the orbits, of more recently discovered asteroids. Soon after the discovery of Pallas by Olbers in 1802, the high inclination of its orbit to the ecliptic was noticed and commented upon; and Pallas is by no means a unique case. Many of the orbits, especially of the fainter asteroids, are pronouncedly eccentric. Thus, Eros, with a period of 643 days, comes at perihelion within the orbit of the Earth, from which it is then distant but thirteen million miles. This tiny object, in fact, is on these occasions our nearest neighbour outside of the Moon. Still more pronouncedly eccentric orbits have been found within the last few years. Thus the tiny body discovered by Dr. Baade at Hamburg in 1920, provisionally labelled 1920 HZ, and later numbered 944, has an orbit of a cometary character, markedly eccentric, with a high inclination. The perihelion is near to the orbit of Mars, while at aphelion the asteroid goes out as far as the orbit of Saturn. It is quite possible, therefore, that many asteroids exist outside of the so-called asteroidal zone, and that their invisibility is due not only to small size but also to great distance. A still closer affinity to a comet exists in the case of the asteroid numbered 224, according to an observation by Señor Comas Sola of Barcelona. On 13 December 1923 this Spanish astronomer photographed the asteroid, which appeared as surrounded by a faint halo not unlike the coma of a comet. This observation has been interpreted as indicating a disintegration of the asteroid.

Several of the asteroids are variable in light. Perhaps the most notable example of a variable asteroid is Eros, which fluctuates in about five and a half hours. If this is due to rotation, evidently the surface of this minute asteroid is so rugged and irregular as to suggest that it is not globular like a planetary body, but simply a huge mass of rock. If this be so and the smallest asteroids are simply rocks and stones moving through space, we have further evidence that there is no real dividing line between asteroids and meteors, and those closely allied bodies which are called comets.

If asteroids were beyond human ken until the beginning of the last century, comets, on the other hand, have been known from time immemorial. In Greek, Roman, and medieval times comets struck terror into the hearts of the people, who believed them to be portents of dire disaster, war, famine, plague. Some of the Greek philosophers held remarkably sane opinions on the nature of these objects. Aristotle however, took the view that they were essentially meteorological phenomena, and this view, adopted later by Ptolemy, prevailed throughout the Middle Ages ; indeed, to question its accuracy was of the nature of heresy. By his careful observations of the comet of 1577, Tycho Brahe demonstrated that that object must be at least six times as distant as the Moon, a conclusion totally at variance with the Aristotelian doctrine, and which proved that comets were celestial bodies.

The next important advance in the study of comets was the demonstration by Newton that, like the planets, they moved according to the law of gravitation. He did not, it is true, prove this in connexion with any given comet. This was reserved for his friend Halley, who had made a special study of the bright comet of 1682. Halley noticed that bright comets had made their appearance at intervals of seventy-five or seventy-six years, and he observed in the case of several of these a close similarity between their tracks. Accordingly, he felt himself safe to predict the return of the comet at the end of 1758 or the beginning of 1759. 'If', he said, 'it should return according to our predictions, about the year 1758, impartial posterity will not refuse to admit that this was first discovered by an Englishman.' The comet was sighted by an amateur astronomer in Saxony on Christmas Day 1758, and thus Halley's prediction was fulfilled.

Posterity has not refused his modest request, and the comet is indissolubly linked with his name. The comet returned in 1835, and again in 1910.

Lunar astronomy, as has been mentioned, was one of the few branches of astronomy in which the work of Herschel did not mark an epoch; cometary astronomy was another. And just as lunar astronomy had a founder in Herschel's contemporary, Schröter, so had cometary astronomy in a slightly younger contemporary, Olbers of Bremen. Olbers was yet another of the remarkable band of German amateur astronomers whose work, towards the close of the eighteenth and during the first half of the nineteenth century, was so fruitful in astronomical discovery. Olbers was by profession a medical practitioner in Bremen, and his work was carried out in the leisure time which he was able to spare from an exacting profession. The upper part of his house in the Sandgasse in Bremen was fitted up as an observatory, in which the largest instrument was a 3¼-inch refracting telescope. Nevertheless, with this instrument he laid the foundation of modern cometary astronomy. Prior to his time the constitution of comets was simply a mystery. But Olbers' careful study of the bright comet of 1811 led him to propound the hypothesis that the tails of comets were formed of minute particles driven out from the head, with a velocity, in the case of this particular comet, equal to that of light. Olbers showed therefore that the tails of comets are not tangible, permanent portions of these bodies, but temporary phenomena visible while comets are in the vicinity of the Sun.

Up to this time only one comet, the famous comet of Halley, was known of a certainty to be a regular member of the Solar System. The first half of the nineteenth century saw a considerable number of comets recognized as belonging to the Sun's family. The first of these was a faint object discovered in 1819 by Pons, at Marseilles, which had been previously seen by Méchain and Caroline Herschel. This comet attracted the attention of Encke, then director of the

Gotha Observatory. He computed its orbit from the observations which had been secured, and found its period to be about 3¼ years. Accordingly, he felt safe in announcing its return in 1822. It was picked up at the Observatory of Paramatta, close to the spot indicated by Encke's calculations. This was the first member to be recognized of a family designated as comets of short period, which are believed to be strongly under the influence of the planet Jupiter.

The second member of the group was discovered by an Austrian officer, Wilhelm von Biela, on 27 February 1826. Observations of its track in the sky made possible the computation of its orbit, and the comet turned out to be periodic, with a period of between six and seven years, with a track which intersected that of the Earth. A considerable scare ensued in 1832 on account of its comparatively near approach to the Earth, which did not subside until it was made clear that at no point would the Earth and the comet approach nearer than fifty millions of miles. On 19 December 1846, Hind, at London, found that the comet was pear-shaped, and ten days later Challis, at Cambridge, noticed to his amazement that it had divided in two. 'I never heard of such a thing', the surprised astronomer wrote to the president of the Royal Astronomical Society. At its next return, both parts of the comet were seen, but the distance between them was greater than in 1846. At the following return, in 1859, the comet was unfavourably placed for observation, and no surprise was expressed that it was not detected. But in 1866, when it ought to have been easily visible, it failed to appear, though eagerly searched for. When next it was seen it was no longer in the form of a comet.

The question of the constitution of comets was forced upon astronomers by the appearance of some very brilliant specimens, among which must be specially mentioned the comets of 1811 and 1843, Donati's comet of 1858, and the great comet of 1861. All of these had striking tails, streaming out for millions of miles, and accordingly the question of comets' tails engaged the attention of astronomers. Zöllner, of Leipzig, developing the hypothesis of Olbers, showed in 1871 that 'the efficacy of solar electrical repulsion relatively to solar attraction grows as the size of the particle diminishes'. Working on this premise, the Russian astronomer, Brédikhine of Pulkova, divided comets' tails into three types, for which, he said, 'the repulsive force is perhaps the same'. The repulsive force on tails of the first type, he ascertained to be twelve times the solar attraction; for tails of the second type, the two forces are equal; while in the case of the third type tails, the force is only a quarter the power of gravity. He formulated the view that the force has the same intensity in all three cases, but the tails are formed of different elements—hydrogen, hydrocarbon, and iron, with a mixture of sodium and other elements. Spectroscopic evidence, however, has not been altogether confirmatory. Hydrogen lines have not been observed in the spectra of comets, and iron lines only on one occasion.

During the nineteenth century, it was generally conceded that the repulsive force was electrical. 'The idea', as Campbell remarks, 'is in part purely speculative, but the giving of serious consideration to it is justified because of the fact that much of the light of comets seems to arise from electrical conditions in them.' However, it is now generally admitted that the chief factor in producing comets' tails is the radiation pressure. The importance of this factor was first emphasized by Clerk-Maxwell in 1873, but it was only at the beginning of the present century that the law of radiation pressure was experimentally verified by the Russian physician, Lebedeff, and the Americans, Nichols and Hull. The small particles of which comets' tails are so largely composed obey the radiation pressure instead of gravity, and comets are thus continually diminishing in bulk through the loss of these smaller particles, which are driven off into space perhaps beyond the confines of the Solar System. Every comet is thus in process of disintegration; its finer material is driven off in great quantities by radiation pressure when it comes comparatively close to the Sun. Probably this is why the short-period comets are, without exception, faint and inconspicuous. ‘ Nearly all of the Jupiter comets ’, as Campbell says, ‘ have a hazy washed-out appearance. Several of them do not develop tails, as if their supply of tail materials had already been exhausted.’

Spectroscopic study of comets’ heads has shown, too, that as comets approach their perihelion they are strongly reacted upon by the Sun. Prior to the invention of the telescope it was generally thought that the heads, at least, shone by reflected light. G. P. Bond of Harvard convinced himself that this was so in the case of Donati’s comet, but Donati himself gave the death-blow to the theory in 1864, when he applied the spectroscope for the first time to a cometary body. The spectrum was not continuous, as would have been the case had Bond’s opinion been correct, but was characterized by bright bands. Donati was unable to identify these ; but four years later Huggins examined the spectrum of Winnecke’s comet and found that the three bright bands represented hydrocarbon. In the same year Huggins found the spectrum of Brorsen’s comet to be composite, namely, three bright bands superimposed on a faint continuous spectrum, indicating that in some cases part of the cometary lustre is due to reflected light.

The hydrocarbon bands, indicating a chemical compound and a comparatively low temperature, are not the only features of cometary spectra. In 1882 the yellow ray of sodium was detected in the spectrum of Wells’ comet by Copeland, at Dunecht, in Aberdeenshire, and this is generally noticed to be characteristic of those comets which approach the Sun very closely. Evidently the bright sodium lines and the bright iron lines are due to the fact that these elements have been rendered incandescent by the intense heat to which the comet is subjected. Spectroscopic observations on a comet discovered by Morehouse, an American astronomer, in 1908, revealed the presence of the lines and bands of cyanogen, a nitrogen compound, and this has been observed in the heads and inner tails of many comets.

According to the recent conclusions of Campbell,

> ' the spectrum of the nucleus, the bright star-like point within the head, is always continuous, or continuous except for absorption lines. In some of the brighter comets the nucleus spectrum as photographed contains the well-known absorption lines emitted in the Sun's spectrum. These observations indicate that the nucleus is shining, at least mainly, by reflected sunlight. . . . The presence of carbon and nitrogen in comets is certain, the presence of oxygen is probable, and the presence of hydrogen is doubtful.'

Spectroscopic work, then, clearly indicates that the comets at perihelion are subjected to intense influences which must hasten their disintegration. That comets do disintegrate was demonstrated in the course of one of the most remarkable investigations in scientific history. Meteors, or shooting-stars, have been familiar from prehistoric times even to the most casual observers of nature. It was believed, however—and much more confidently than in the case of comets—that they were atmospheric phenomena pure and simple, and until the beginning of the nineteenth century they do not appear to have been systematically studied at all. In the year 1799 a remarkable shower of shooting-stars was visible chiefly in South America ; it was observed by Humboldt, who left a description of it, but his description does not seem to have attracted any considerable notice. It was not until 1833 that the attention of astronomers was definitely turned to the subject. On the night of 12–13 November a magnificent shower of meteors was witnessed, more particularly in America, where the rain of stars lasted all night through. The display was carefully watched by two American observers, Olmsted and Twining, who ascertained that the paths of all the meteors when traced backwards were found to converge to a point in the sky near the star Gamma Leonis. This was a test observation ; it proved beyond a doubt that the meteors

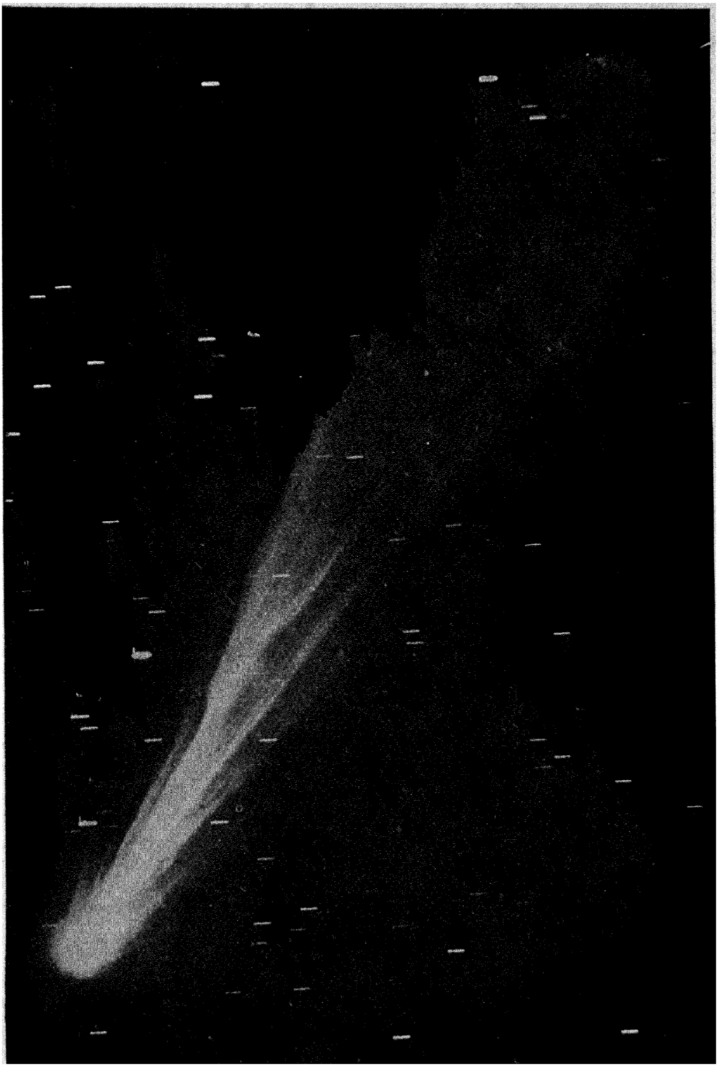

FIG. 18. MOREHOUSE'S COMET, 1908

From a photograph by Greenwich Observatory were not atmospheric but cosmical in nature and in origin. The acute mind of Olbers was brought to bear on the question, and he suggested that as a shower had been seen thirty-four years earlier, one might be expected thirty-four years later, in 1867.

As the year 1867 drew near an American astronomer, H. A. Newton of Yale, searched through all the records of meteorological phenomena available to him in order to test this suggestion of a thirty-three year period. His search confirmed the surmise, and accordingly he predicted a great shower on the night of 13-14 November 1866. His prediction was fulfilled to the day. The shower was inferior to that of 1833, but it was a magnificent spectacle nevertheless. Newton attempted to determine the orbit of the hypothetical swarm of particles producing the shower. He was unable to do so, but shortly afterwards, Adams, the co-discoverer of Neptune, proved that the period was one of 33½ years, and the swarm of Leonid meteors revolved round the Sun in a highly elliptical orbit, the aphelion of which lay near to that of Uranus.

Meanwhile, the keen mind of Schiaparelli had been engaged on the question, and in four letters addressed to Secchi, director of the Observatory of the Collegio Romano, towards the close of 1866, he recapitulated the assured facts about meteors. Meteors, he showed, were members of the Solar System, moving in certain directions with greater velocity than the Earth along tracks resembling those of comets in their eccentricity and inclination. Further, Schiaparelli announced that in one case, at least, a meteor track not only resembled but was identical with a cometary orbit. Having computed the orbit of the Perseid or August meteors, he found that it was the same as that of the comet of 1862. Early in 1867 he found that Le Verrier's revised elements for the orbit of the Leonids were identical with those of Tempel's comet of 1866. Peters of Altona independently reached the same conclusion, while Weiss of Vienna drew attention to the similarity of the orbit of a star-shower on or near 20 April and that of the comet of 1861. He also pointed out, independently of Galle and D'Arrest, the German astronomers, the close similarity between the orbit of a second November meteor-shower, the Andromedids, and Biela's lost comet.

All doubt as to the connexion between comets and meteors was removed by the great shower of 27 November 1872. Biela's comet was again due at perihelion at this time, and although again searched for with diligence was not seen. As Weiss and Galle surmised, however, a brilliant meteoric display ensued—so brilliant as to be described by Denza, an Italian observer, as 'a real rain of fire'. Evidently the Earth was ploughing its way through the disintegrated remains of Biela's comet; a comet, then, is simply a swarm of meteoric particles, more or less closely packed together.

Schiaparelli, in a course of lectures delivered in 1873 and afterwards reproduced in book form, outlined the theory of the disintegration of comets into meteors. He concluded that comets are cosmical clouds formed in space by 'the local concentration of celestial matter'. A cosmical cloud, however, will not penetrate to the interior of the Solar System 'unless it has been transformed into a parabolic current', which may occupy years or centuries in passing its perihelion, 'forming in space a river, whose transverse dimensions are very small with respect to its length; of such currents, those which are encountered by the Earth in its annual motion are rendered visible to us under the form of showers of meteors diverging from a certain radiant'. 'The meteoric currents are the product of the dissolution of comets, which is brought about by the action of the Sun and planets on the different particles composing the heads of comets, which are thus drawn into different orbits.'

That these meteor swarms exist in immense profusion has been made evident by the large numbers of 'radiants', i.e. points in the sky from which meteors radiate, which have been discovered during the last half-century. The first catalogue was drawn up by Heis of Munster in 1867; it contains eighty-four radiants, representing eighty-four different swarms. A second catalogue was issued by Schiaparelli in 1871. Based largely upon the observations of an Italian observer, Zezioli, it contains 189 radiant points. Among later catalogues the most important was that of W. F. Denning of Bristol, an English amateur, who gained for himself the premier place among observers of meteors. Denning's catalogue, published in 1899, contained 4,637 radiants. Dr. C. P. Olivier, the chief American student of this branch of the science, has brought forward weighty reasons for reducing the number to 1,200, at least one-half of which 'represent one or more appearances of real streams'. These, it should be remembered, are only the meteor swarms whose orbits happen to intersect the orbit of the Earth. Nor do they include the vast number of isolated meteors which do not radiate from any common point; nor again, do they include the larger meteorites, which from time to time fall to the surface of the Earth.

From the earliest ages traditions have been preserved of stones having fallen from the sky—for example, the sacred stone of Mecca and the 'image which fell down from Jupiter', referred to in the account in the Book of Acts of the riot at Ephesus on the occasion of Paul's visit, and evidently held in special reverence by the Ephesians. Nevertheless, as late as 1800 the French Academy adopted the definite opinion that all stories of stones falling from the sky were absolutely untrue. It was not until 1803, when a fall of stones was actually witnessed at L'Aigle, in the department of the Oise, that scientific prejudice was overcome. Many instances of falling stones have since been recorded; opinions have differed considerably as to whether these are the larger members of ordinary meteor swarms, or whether they form a class by themselves. The fact that on only one occasion was a meteorite seen to fall during a great shower would seem to be evidence against their close connexion. Schiaparelli held strongly to the view that, while akin in many respects to comets and meteors, these large meteorites come from interstellar space, and this conclusion would seem to be borne out by the work of Hoffmeister, a careful German observer.

Two alternative theories of the place of origin of comets have contended for mastery since the foundation of cometary astronomy. Newton maintained that comets are native-born members of the Solar System, moving in elongated ellipses. Laplace, at the close of the eighteenth century, formulated an alternative theory to the effect that comets are visitors from interstellar space. He based this on the fact that most cometary orbits appeared to be parabolic. In the case of a comet, such as Halley's, Laplace believed it to have been captured by the Sun, and to have thus become almost accidentally a member of the Solar System.

Since the time of Laplace many different pieces of evidence have been collected, and have undermined his theory. A considerable number of comets were discovered to be periodic during the nineteenth century; it was, in fact, found that a comet family was associated with each of the four giant planets. Schiaparelli showed, about fifty years ago, that if comets really come from interstellar space, their orbits should be not parabolic, but strongly hyperbolic, and no known cometary orbit falls into that category.

Campbell pointed out in 1918 that 'of 347 comet orbits fairly well determined, 60 are certainly elliptic, 275 are approximately parabolic, 12 or fewer are slightly hyperbolic, and none are strongly hyperbolic'. The work of Thraen, Fayet, and Fabry has shown that several of the twelve hyperbolic orbits are not really hyperbolic, but only became so under the disturbing influence of the planets. Fayet, too, has demonstrated that the vast majority of the so-called parabolic orbits are only apparently so when the comets are close to the Sun, while when such comets are observed at great distances from the Sun the orbits are clearly elliptic. A statistical study of comet orbits by the American astronomer Leuschner showed that before 1755, 99 per cent. of comets were said to move in parabolic orbits, while between 1846 and 1895 only 54 per cent. were thus classified.

> 'These facts', as Campbell maintains, 'point to the conclusion that when comets are observed inaccurately, as of old, and in only a short section of their orbits, parabolic orbits satisfy the observations within the limits of the error unavoidably attaching to those observations; but that when comets are observed accurately and for a long stretch of time, nearly all are found to be moving in ellipses. Most of the ellipses are of course extremely long ones.'

The aphelion distances of comets are certainly very great compared with the distances of the planets, but they are still well within the domain of the Sun. Indeed, if we except the short-period comets which have been apparently captured under the influence of the giant planets, it may be said that most comets spend the greater part of their time in the outer region of the Solar System. Probably the vast majority of such bodies escape observation altogether from Earth, and many of them probably move in orbits only slightly elliptical. The outer regions of the Solar System must, therefore, be thickly populated by comets, and, in the opinion of Dr. See, comets are 'survivals of the ancient nebula out of which the Solar System has been built up'.

Possibly another survival, in all likelihood closely connected with cometary and meteoric matter, is a faint haze of light which streams from the Sun along the ecliptic, and is occasionally to be seen after sunset and before sunrise in northern latitudes. To this haze the name of the Zodiacal Light was given by Cassini in 1653. The fact that it lies along the ecliptic indicates that it forms some sort of luminous appendage of the Sun. Brorsen, a German astronomer, discovered, in the last century, a fainter light in the opposite region of the sky to the place occupied by the Sun. This light he named the 'Gegenschein', or 'counter-glow', and its existence was confirmed in 1883 by Barnard, who, indeed, discovered it independently, as he was previously unaware of

Brorsen's discovery. Quite evidently, the 'Gegenschein' represents that part of the Zodiacal Light beyond the Earth's orbit. The theory which appears most reasonable, and is most generally accepted, is that the light and counter-glow are composed of myriads of minute meteoric bodies, some perhaps of asteroidal dimensions, mixed up with cosmic dust. Some years ago, Dr. Van Rhijn of Groningen, in his study of the brightness of the sky, concluded that 'at least part of the light of the sky proceeds from a source probably identical with that of the Zodiacal Light'—'a kind of Zodiacal Light extending over the whole sky'. The Solar System would seem, then, to be enwrapped in a kind of ghostly nebulosity, composed of meteoric particles and fine dust, which evidently represents equally with asteroids and comets the surplus matter left over in the making of the Sun and planets.

VII

THE STARS

THAT the stars are suns, and that conversely the Sun is simply our nearest star, was recognized over three centuries ago as the necessary corollary of the Copernican system of the world. The name of 'fixed stars' lingered on, it is true; but the discovery by Halley in 1718 that Sirius, Arcturus, Betelgeux, and Aldebaran had perceptibly shifted their positions in the heavens since the time of Ptolemy, indicated clearly that the designation was a misnomer. 'There is not, in strictness of speaking,' said Herschel in 1783, 'one fixed star in the heavens.'

'If the proper motion of the stars in general', wrote the same great astronomer, 'be once admitted, who can refuse to allow that the Sun, with all its planets and comets, that is the Solar System, is no less liable to such a general agitation as we find to obtain among all the rest of the celestial bodies ? ' It occurred to Herschel that the motion of the Sun through space might be detected through a resulting drift of the stars, just as the orbital motion of the Earth is reflected in certain components of the planetary motions. If the Sun is moving in a certain direction the stars in front will appear to disperse, while those behind will seemingly draw closer together. But the problem is complicated by the individual motions of the stars ; and the proper motions, themselves very minute, have to be decomposed into two parts, the real motions of the stars themselves and the apparent motion common to all the stars, which results from the translation of the Solar System. ' We ought, therefore,' said Herschel, ' to resolve that which is common to all the stars which are bound to have what has been called a proper motion into a single real motion of the Solar System, as far as that will answer the known facts, and only to attribute to the proper motion of each particular star the deviations from the general law the stars seem to follow in those movements.'

Herschel treated the problem in the simplest manner. Dealing with the proper motions of seven bright stars—Sirius, Castor, Procyon, Pollux, Regulus, Arcturus, and Altair—he separated their real from their apparent motions by simple geometrical methods, and reached the conclusion that the Solar System was moving towards a point in the constellation Hercules, the ' apex ' of the solar motion being marked by the star Lambda Herculis. ' We may ', he said, ' in a general way estimate that the solar motion can certainly not be less than that which the Earth has in her annual orbit.' In 1805 Herschel again attacked the problem, making use of Maskelyne's table of the proper motions of thirty-six stars. His result was in the main confirmatory of his earlier conclusions, the ' apex ' being again located in the constellation Hercules. But Herschel's discovery was regarded with considerable incredulity by several of his contemporaries and successors. They seemed to feel that the data on which he worked were too slender for a trustworthy result to be deduced. Bessel, the greatest practical astronomer of the next generation, maintained that there was no evidence in favour of a motion towards a point in Hercules, and even Sir John Herschel rejected his father's conclusions.

From the time of Copernicus downward the problem of the distance of the stars occupied the attention of astronomers. It was early recognized that the parallaxes of the stars—that is, their displacement on the celestial sphere due to the Earth's motion—from which the distances could alone be deduced, must be exceedingly minute, and in each generation efforts were made to measure this minute quantity, but without success. Bradley, the foremost practical astronomer of his age, was unsuccessful, and Herschel, even with his much greater telescopic power, was not more fortunate. And so the astronomer who laid the foundations of modern stellar astronomy was ignorant of the unit of distance in the stellar system. Some of his estimates of the scale of the system were, however, remarkable approximations to the truth. The detection of quantities so minute as the proper motions and parallaxes of even the nearest stars came about as the result of a rapid development of instrumental refinement just after Herschel's time, and as the consequence of the labours of a number of distinguished German astronomers, among whom the outstanding figure was Friedrich Wilhelm Bessel.

While the reflector was in the heyday of its prestige, the refracting telescope was steadily making way. Dollond, the English optician, had shown in 1758 that chromatic aberration could be overcome by the combination of lenses of flint and crown glass. For many years, however, it was found impossible to procure flint-glass except in very minute fragments. But about the very time when Herschel was engaged on his first telescopes, a Swiss tradesman, Pierre Louis Guinand, of the canton of Neuchâtel, began to amuse himself by grinding spectacles, and afterwards lenses. His attention being turned to telescope-making, he conceived the idea of constructing an achromatic telescope—that is to say, a telescope free from chromatic aberration. Experiment after experiment failed, but finally he succeeded in 1799 in fashioning several discs four to six inches in diameter. Nearly twenty years later he achieved a still greater triumph—the manufacture of perfect discs up to eighteen inches. In 1805 Guinand was invited to Munich, where, in the previous year, Von Reichenbach had established an optical and mechanical institute. Here Guinand became the instructor of Fraunhofer, whose work in the perfecting of telescopes, during his lifetime at all events, earned for him a wider fame than his preliminary spectroscopic work. In 1817 Fraunhofer succeeded in completing the finest refracting telescope in the world. The object-glass measured 9½ inches in diameter, and was for some time the greatest in the world; more important still, the telescope moved by clockwork to keep pace with the apparent diurnal motion of the stars. This fine instrument was placed in the hands of Friedrich Georg Wilhelm Struve, the young German astronomer who had gone to Dorpat, in Russia, to direct the new observatory there, and was used by him in his attempt to measure parallax, and in his work on double stars. Still more potent an engine in astronomical advance was the heliometer which Fraunhofer constructed for the Observatory at Königsberg. This instrument might be more accurately termed the divided object-glass micrometer. Instead of one single lens the object-glass consists of two half-lenses, by means of which two images of the same object are formed.

Fortunately for astronomical progress this instrument passed into the hands of the greatest practical astronomer of the day. Friedrich Wilhelm Bessel, the son of a civil servant at Minden on the Weser, was originally a clerk in a commercial house in Bremen. While so employed he took up the study of science, and found astronomy specially fascinating. His computation of the orbit of Halley's comet from some old seventeenth-century observations brought his work to the notice of Olbers, and thus before the scientific world. Within a decade Bessel had attained to one of the highest posts in Europe—that of director of the new Observatory at Königsberg, where he laboured till his death in 1846. He left a permanent mark both on practical and descriptive astronomy. By his reduction of the observations made by Bradley from 1750 to 1762, a process which involved much mathematical refinement, Bessel made those observations of star-positions at the middle of the eighteenth century the starting-point of modern statistical astronomy.

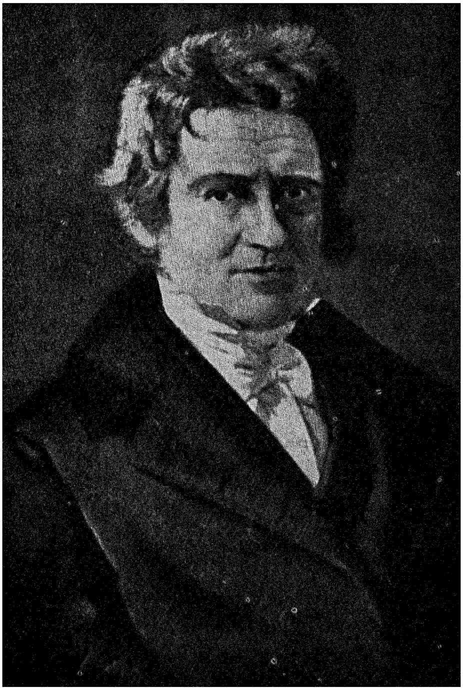

FIG. 19. FRIEDRICH WILHELM BESSEL

Further, he carried through during the years 1821 to 1835, with the greatest precision, a survey of 75,011 stars. Thus the accurately known stars were increased to over 50,000, and an enormous amount of material was thus accumulated for the use of future generations. In this line of work Bessel's successor was Argelander, who was for a time his assistant before his appointment to the Observatory at Bonn. During the years 1852–63 Argelander carried through his great *Durchmusterung*, a catalogue and atlas of 324,198 stars visible in the northern hemisphere, a piece of work which was later extended by his successor, Schönfeld, to embrace 133,659 stars in the southern hemisphere.

Two highly important results flowed from this rapid development of instrumental power. In 1837 Argelander succeeded, as a result of the study of 390 proper motions, in placing beyond all doubt the proper motion of the Sun, and thus confirming Herschel's first conclusions which had been rejected by his son, Sir John Herschel, as founded on insufficient data. In 1838 three simultaneous attacks were made on the problem of star-distance. Struve's estimated parallax for Vega, measured with the Dorpat refractor, was considerably too large, and did not inspire much confidence; but the heliometer in the skilful hands of Bessel himself wrested the secret of its distance from a faint star of the fifth magnitude, numbered 61 Cygni. In December 1838 Bessel announced that the parallax of 61 Cygni was a little over three-tenths of a second of arc, corresponding to a distance 600,000 times greater than the Earth's distance from the Sun, or round about forty million miles—a result substantially confirmed afterwards by Otto Struve and later astronomers.

Before Bessel commenced his work on 61 Cygni, a less famous astronomer was engaged on an equally important attack on Alpha Centauri, the third brightest star in the sky. Thomas Henderson, originally a lawyer's clerk in his native town of Dundee, and afterwards in Edinburgh, served as His Majesty's Astronomer at the Cape of Good Hope from

1832 to 1834, when he returned to Edinburgh as Astronomer Royal for Scotland. While in South Africa he carried out a series of measures on Alpha Centauri. His instruments were not of the finest, but this handicap was counterbalanced by his remarkable skill as an observer, and by the proximity of the star. For some reason he did not reduce his observations until after his return to Scotland, and it was not until January 1839 that he was able to announce that Alpha Centauri showed a parallax of about a second of arc, corresponding to a distance of twenty billion miles. By this time his friend Bessel had forestalled him by a few weeks. Henderson's measures have been substantially confirmed by later astronomers ; they were only slightly in error. The distance of this our nearest stellar neighbour is known to be very nearly twenty-five billions of miles, a distance which light takes about four years to travel. Soon afterwards Maclear, Henderson's successor at the Cape, measured the parallax of Sirius, the brightest star in the sky, which is fifty billions of miles away, more distant than the fainter 61 Cygni. From these early parallax measures one highly important fact was ascertained, namely, that the stars were by no means equally bright, but differed enormously among themselves in actual luminosity.

Progress in the measurements of stellar parallax was, however, disappointingly slow during the nineteenth century, and the same may be said of the measures of proper motion. After the middle of the century, the centre of gravity in stellar astronomy was shifted to stellar spectroscopy. The first analysis of starlight by means of the spectroscope was made in 1860 by Donati at Florence. His instrumental means, however, were very limited, and beyond fixing the position of the more important lines he was unable to advance very far. The first effective steps in the conquest of this new department of stellar astronomy were made by William Huggins, a London amateur, who afterwards became the greatest English astronomer of his day, and by Angelo Secchi, a Jesuit priest, who filled the post of director of the Observatory of the Collegio Romano. Huggins concentrated on the intensive study of bright stars, and in 1863 he announced the preliminary results of his investigation of Betelgeux and Aldebaran. In these stars he identified the familiar elements of sodium, iron, calcium, bismuth, and magnesium.

A few years later, in 1868, Huggins applied Doppler's principle in an attempt to measure the motions of the stars in the line of sight. ' It would scarcely be possible ', wrote Huggins, to convey ' any true conception of the difficulties which presented themselves in this work, from various instrumental causes, and of the extreme care and caution which were needed to distinguish spurious instrumental shifts of a line from a true shift due to the star's motion.' At length Huggins was in a position to announce that Sirius was retreating from the Solar System with a velocity of twenty-nine miles per second; and some time later he announced that Betelgeux, Rigel, Castor, and Regulus were retreating, and Arcturus, Pollux, Vega, and Deneb approaching. Huggins' work was largely pioneer work, and more solid results were achieved by Vogel, who in 1887 applied photography to the measurement of radial motions. Later on, the American astronomers, Keeler, Campbell, Frost, and others, carried out extensive measures of radial velocities, and Campbell was able to deduce the solar motion from his measures of the radial motions of hundreds of stars.

Instead of concentrating on a few stars Secchi made a spectroscopic survey of the heavens, which occupied him for four years. Four thousand stars were passed in review, and Secchi found that these fell into four distinct types. The first type, represented by Sirius, Vega, Altair, and other bluish-white stars, have spectra characterized by the intensity of the hydrogen lines ; the second type, embracing the yellow stars such as Capella, Arcturus, Pollux, and the Sun itself, were found to have spectra showing innumerable metallic lines. Secchi's third type included Betelgeux and Antares and other red stars, and their spectra were found to be characterized by strong absorption bands. The fourth type comprised scarce red stars, also with broad absorption lines. To Secchi's four types, a fifth, consisting of comparatively few stars, was added in 1867 by Wolf and Rayet, of the Paris Observatory. These Wolf-Rayet stars show spectra dominated by bright lines in place of dark. At first Secchi's classification was regarded as more or less empirical and arbitrary, though latterly he was led to the opinion that it 'represented real physical conditions varied by the temperatures prevailing on the different stars'.

In 1865 the German astronomer Zöllner, of Leipzig, casually suggested that yellow and red stars were simply white stars in different stages of cooling, and this idea formed the basis of the elaborate classification of the stars which we owe to his distinguished pupil Vogel, afterwards of Potsdam. 'A rational classification of the stars according to their spectra', Vogel wrote in 1874, 'is probably only to be obtained by proceeding from the standpoint that the phase of development of the particular body is in general mirrored in its spectrum.' Vogel retained the general framework of Secchi's classification, holding that the white stars, such as Sirius and Rigel, were the hottest and youngest stars, yellow stars, such as the Sun and Capella, being obviously older and cooler. He viewed red stars as still older and much cooler, as 'effete suns hastening rapidly down the road to final extinction'. Vogel revised his classification in 1895, in order to make room for the newly-recognized group of blue or helium stars, which he considered to be still hotter and more immature than the suns of the Sirian variety. In the main, Vogel's scheme and its underlying evolutionary assumption was adopted by Huggins, and formed the basis of the elaborate 'Draper classification', carried through at Harvard, U.S.A., under the direction of the late Professor E. C. Pickering. This classification is based on the observed spectral lines, and is at the present time in general use. In this scheme, the various classes of stars are designated by the letters OBAFGKMN, type O consisting of Wolf-Rayet stars, B and A of the Sirian type, F, G, and K of stars of the solar type, and M and N corresponding to the two classes of red stars. This sequence, OBAFGKMN, was, until about ten years ago, believed by the great majority of astronomers to represent the order of stellar evolution.

As has been mentioned, the rate of progress in the measurements of stellar distances was, during the nineteenth century, disappointingly slow. In Newcomb's list of parallaxes appended to his volume on *The Stars*, published in 1901, only fifty-eight were classed as reasonably reliable. During the two decades of the present century, however, unparalleled progress has been made. In 1915 Sir F. W. Dyson estimated that the distances of about two hundred stars were known with some degree of accuracy; and in the intervening years many hundreds of parallaxes have been successfully measured. The result of this advance—due chiefly to the great American observatories—is that the distances of 1,400 stars are now known with considerable accuracy from trigonometrical measures alone. Still more remarkable has been the fact that other methods of measuring stellar distances have been devised, one of which at least can compete in accuracy with the older method. In 1916 Dr. W. S. Adams, of the Mount Wilson Observatory, outlined the method of determining parallaxes by means of the spectroscope. In conjunction with Dr. Köhlschutter, a young German astronomer who was for some years engaged in research work at Mount Wilson, Dr. Adams detected a relationship between the absolute brightness of stars and the intensity of certain lines in their spectra. Thus from the examination of certain lines, the absolute magnitude of a star may be found and its distance determined.

'In five years', says Professor Hale in a recent report, '2,000 stellar parallaxes have been determined at Mount Wilson by this beautiful process, which has been applied with the 100-inch reflector to stars as faint as the ninth magnitude, and could be pushed to much fainter objects. Thus, while the value of trigonometrical parallaxes is by no means diminished, but rather increased by the introduction of the spectroscopic method, the range of action and the rate of progress have been advanced in very high degree.'

The first authentic measures of stellar distances revealed the fact that the stars differed considerably in luminosity and presumably in size and mass. Thus Henderson's measures of the distance of Alpha Centauri indicated that our nearest neighbour is brighter than the Sun, while Bessel's value for 61 Cygni made that star much fainter. As parallax measures accumulated, still greater differences in luminosity manifested themselves. It was not, however, until 1905 that there was any recognition of the fact that two main groups were to be traced among the stars. In that year Dr. Hertzsprung, of Potsdam, pointed out, as the result of his statistical work on stellar parallaxes, masses, and luminosities, that the stars appeared to belong to one or other of two well-defined classes—large diffuse stars and comparatively small stars of greater density. To these two classes he gave the names of 'giants' and 'dwarfs'. This discovery was made independently by Professor H. N. Russell. From data concerning the masses and brightness of the stars, compiled in the course of investigations of stellar parallax, Russell recognized the existence of giants and dwarfs among the red stars. 'There are', he wrote in December 1913, 'two great classes of stars—the one of great brightness, averaging, perhaps, a hundred times as bright as the Sun, and varying little in brightness from one class of spectrum to another; the other of smaller brightness, which falls off rapidly with increasing redness.'

More recent research has placed the existence of giant and dwarf stars beyond doubt. The work of Hertzsprung indicated that even among the 'early types' two classes of stars were to be distinguished. Striking confirmation was obtained by Adams in the course of his determination of stellar parallaxes by the new spectroscopic method. In 1916 Adams announced that 'two groups of M stars are indicated clearly by an examination of the intensity of the hydrogen lines; in the first the hydrogen lines are very strong; in the second they are very faint. . . . Connecting links over a range of seven magnitudes are entirely lacking'. About the same time Dr. Shapley detected in the distant star-clusters red and yellow stars, which, to be visible at all at such vast distances from our system, must be giants. He also noticed that in some of the clusters the brightest stars are red—a striking confirmation of Russell's views.

Perhaps the most convincing of all the pieces of evidence is the information gained during the past decade as to the actual sizes of certain stars. In 1916 two very faint stars of rapid proper motion were discovered, one by the late Professor Barnard, of Yerkes Observatory, and the other by Dr. Innes, in South Africa. The small star in Ophiuchus detected by Barnard proved to be one of the Sun's nearer neighbours in space; its luminosity appears to be one two-thousandth of that of the Sun, and its diameter three-ninths. Dr. Innes' star is still nearer to our system, and is presumably our nearest neighbour in space; it has been designated Proxima Centauri, and is evidently physically connected with the Alpha Centauri system. In luminosity and size it is similar to Barnard's star. Dr. A. C. D. Crommelin has remarked that it is 'in all probability very near the end of its career as a star'. These stars are indisputably dwarfs.

Still more striking evidence resulted from the remarkable feat of the Mount Wilson astronomers in measuring the angular diameters of certain stars. This was due to the interferometer, an instrument first applied to astronomy by Professor Michelson, of Chicago. This consists in principle of an opaque disc, large enough to obscure the object-glass or mirror of a telescope, and pierced with two small holes, whose distance from each other can be varied. When it is placed in front of the telescope—the telescope being directed towards a star—the image of the star, which is formed by light coming through the holes, is crossed by several dark lines which are called interference fringes. If the holes are separated gradually the fringes become more and more indistinct, and finally disappear if the telescope is large enough. From a relation between the separation of the holes and the wave-length of the light from the star the angular diameter can be measured, and if the parallax is known the linear diameter can be computed. There are very few stars large enough for the fringes to disappear, and, conversely, very few telescopes large enough to show the disappearance. On the night of 13 December 1920, however, Messrs. Pease and Anderson turned the largest telescope on the Earth to one of the largest stars in the sky. The angular diameter of Betelgeux came out as 0·045″, in close accordance with Eddington's theoretical value announced a few months before. As the parallax has been measured with some degree of accuracy, the linear diameter came out at 273 million miles. Thus, if Betelgeux were in the centre of the Solar System in place of the Sun, the Earth's orbit would be wholly within the star's circumference. In point of fact, the volume of the giant of Orion is 27 million times that of the Sun. Later, the diameters of Arcturus, Aldebaran, and Antares were measured, and came out as 21 million, 270 million, and 400 million miles respectively. More recently, the existence of still greater giants has been strongly hinted at by the work of Shapley. In 1923 he announced that some of the stars in the Nubecula Minor were in all probability about a thousand million miles in diameter.

Among the Sun's neighbours the dwarf stars appear to be considerably more numerous than the giants. A moderate estimate makes them number three-fourths of all the stars. In a list of the twenty stellar neighbours of the Sun—stars distant not more than five parsecs [1] from the Solar System—there is not a single giant star ; all are dwarfs, and what is still more remarkable, only four—Sirius, Altair, Procyon, and Alpha Centauri—exceed the Sun in absolute magnitude. The remainder are considerably fainter, and presumably smaller, and most of them are invisible to the unaided eye. It is not until we get beyond the Sun's immediate neighbourhood that we come upon the more familiar orbs of our night skies. The nearest giant star appears to be Vega, about thirty light-years away; while Antares and Betelgeux, the super-giants whose diameters have been recently measured by means of the interferometer, are distant over a hundred light-years. At still greater distances are the familiar cluster of the Pleiades, and the bright groups of Ursa Major and Orion stars. These appear to be at least six hundred light-years away.

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[1] A parsec is the linear distance corresponding to a parallax of one second of arc.

Consideration of the list of the Sun's immediate neighbours reveals another significant fact. At least one-half are binary stars—systems in which the fainter star may be said to revolve round its primary, or more accurately, in which both stars may be regarded as revolving round their common centre of gravity. Double stars have been familiar objects from the early days of telescopic astronomy. In 1650, Riccioli, an Italian observer, noticed that Mizar in Ursa Major, when seen through the telescope, consisted of two stars. Six years later Huyghens made a similar discovery in regard to Theta Orionis, and in 1664 Hooke, the English astronomer, found a third double star in Gamma Arietis. Later, in the seventeenth century, the two brilliant southern stars, Alpha Gruis and Alpha Centauri, were likewise found to be double. At this period, however, double stars were looked upon more or less as curiosities. They were generally believed to be double only in appearance, owing to the fact that the fainter star happened to lie in the same line of sight as the brighter, at a much greater distance. One or two sagacious thinkers, such as the Englishman, John Michell, and the German, Christian Mayer, pointed out the extreme improbability of a chance scattering of stars resulting in the close juxtaposition of as many pairs as were then known. But it was reserved for Sir William Herschel, as a result of his long series of observations, to demonstrate that a certain proportion of double stars were actually systems of suns. In 1803 he was in a position to announce that in the case of six stellar pairs the two stars were in mutual revolution. These Herschel designated as ‘ binary sidereal systems ’.

Perhaps the most dramatic discoveries in this department after the time of Herschel were that of companion stars to the bright stars Sirius and Procyon. In 1844 Bessel détected irregularities in the proper motions of both of these stars, and suggested that in both cases these were due to the presence of faint satellites of large mass. In 1862 a faint companion to Sirius was discovered, and in 1896 a faint companion to Procyon. Both of these companion stars were, like Neptune, discovered before they were seen.

In the study of double stars much progress was made in the course of the nineteenth century. Sir John Herschel, F. G. W. Struve, and his son, Otto Struve, and in the later decades of the century the late Professor S. W. Burnham of Chicago, detected great numbers of new doubles. More recently, Messrs. Aitken and Hussey, of the Lick Observatory, have still further increased the sum-total of known double and presumably binary stars. Of these a certain proportion are triple and quadruple. Several thousands of telescopic double stars are now known, and the orbits of 112 of these had been computed up to the end of 1917. Of these, the periods range from a little over five and a half years to about five hundred years.

A new epoch in this department of astronomy was opened in 1889 by the detection by the late Professor E. C. Pickering of the first spectroscopic binary star. The Doppler principle was first applied to the study of stellar motions as far back as 1868, but it was only after the application of photography to stellar spectroscopy that it yielded reliable results. No only have the radial velocities of large numbers of stars been measured—that is to say, the component of velocity in th line of sight—but an entirely new class of double star has been discovered. The variability of the radial motion, the existence of what Professor Campbell defines as 'radial motions alternately of approach and recession with reference to the velocity of the system as a whole', reveals the fact that a star, which in the most powerful telescope appears as a single body, is in reality composed of two bodies, which are in many cases nearly equal in mass, though often very unequal in luminosity. The first star thus detected as double was the brighter component of Mizar or Zeta Ursa Majoris, which was also the first visual double to be detected. A few months later it was announced from Harvard that Beta Aurigae was a binary system of the same kind, while in the same year, Vogel, at Potsdam, made the same discovery in regard to Spica, the bright first-magnitude star in the constellation Virgo.

By the close of the nineteenth century thirteen of these spectroscopic binary systems were known; and since 1900 large numbers of stars have been found with irregular variable motion. The work of Campbell at the Lick Observatory and Frost at the Yerkes Observatory has resulted in hundreds of discoveries, until by 1918 about six hundred spectroscopic binaries were catalogued. These include a type of body which is also classed as a variable star. It is obvious that if the plane of the orbit of a spectroscopic binary happens to lie in the line of sight of the terrestrial observer, the two stars will alternately eclipse each other, either totally or partially. Such stars were known—long before the existence of the spectroscope—as variable stars.

The first 'variable star' was discovered in 1598 by the Dutch observer, Fabricius. This was Omicron Ceti, the most thoroughly investigated of the long-period variables, which Hevelius called Mira, or the wonderful star. In 1669 Montanari, an Italian observer, discovered that Algol, or Beta Persei, was unstable in light; but these discoveries attracted little attention. It was reserved for Herschel to focus attention on the vagaries of Mira, and for Goodricke, an English amateur, who died at the early age of twenty-one, to rediscover in 1782 the light-changes of Algol, and incidentally to detect the variability of Beta Lyrae and Delta Cephei. Goodricke was the first to suggest the true cause of the variations of Algol, but the suggestion received little attention until Pickering, in 1880, as the result of rigorous examination of the star's light-curve, placed it beyond reasonable doubt. In 1889 Vogel tested the theory by the spectroscope, and found the two stars to be in motion round their centre of gravity.

That fundamental differences exist among the variable stars was noticed at an early period. And when in 1880 Pickering put forward his classification of variables he recognized this fact. In his first class he included temporary stars; in his second and third classes he included the long-period variables, such as Mira Ceti, and the irregular variables such as Betelgeux; his fourth and fifth classes comprised the variable stars of short period—Beta Lyrae, Delta Cephei, and Zeta Geminorum in the fourth class, and Algol in the fifth. At a later date the Beta Lyrae type of variable was recognized to be essentially similar to the Algol stars, while a distinction was drawn between the 'Cepheid' and 'Geminid' varieties. In 1895 another sub-class was added by Professor Bailey of Harvard, who detected in that year the 'cluster variables', a type closely akin to the Cepheids and Geminids.

In his *Study of the Orbits of Eclipsing Binaries*, published in 1915, Dr. Shapley discussed the variations of ninety Algol variables, including those of the Beta Lyrae type, and these variations found their explanation in every case in the binary nature of the stars. These stars are variable because their orbits happen to lie in our terrestrial line of sight. Seen from different parts of the stellar universe, all the spectroscopic binaries are variables. Indeed, Dr. Shapley has remarked on the curious fact that

'the Sun is also an eclipsing system. To an observer on a star located in the immediate vicinity of the plane of Jupiter's orbit, there is a solar eclipse every twelve years of more than a hundredth of a magnitude—a variation possibly discernible with a modern photometer. This minimum—an annular eclipse of a darkened disc—would be nearly three times as deep at zero phase as it would be near the solar limb. Similarly, of course, there are eclipses for the other planets, but the most favourable conjunction of all of them would hardly double the depth of the minimum due to Jupiter alone.'

Observations of Delta Cephei and Zeta Geminorum by Bélopolsky at Pulkova in 1894 and 1895 indicated that these stars, like the Algol variables, were characterized by variable radial velocity, and accordingly the view was widely adopted —somewhat hastily—that all short-period variable stars were actually binary systems. The remarkable fact was early noticed, however, that, unlike the Algol variables, the maximum brightness of these stars occurs near the point of maximum velocity of approach, a fact which is very difficult of explanation on the binary hypothesis. Accordingly, theory after theory was put forward in the early years of the century to explain the anomalies of these remarkable stars; and on the binary hypothesis the absurd result was reached that the radii of the stars' orbits averaged less than one-tenth the radii of the stars themselves.

In August 1914 Dr. Shapley published an important paper on *The Nature and Cause of Cepheid Variation*. 'It seems a misfortune, perhaps,' he remarked, 'for the progress of research on the causes of light-variation of the Cepheid type that the oscillations of the spectral lines in nearly every case can be so readily attributed by means of the Doppler principle to elliptical motion in a binary system.' After pointing out the essential identity of Cepheid, Geminid, and cluster variables, Dr. Shapley concluded that these 'are not binary systems, and that the explanation of their light-changes can much more likely be found in a consideration of internal or surface pulsations of isolated stellar bodies'. 'We may suppose that, because of the internal vibration, the photosphere of the star is periodically scattered or broken through by the rush of hotter gases from the interior. Maximum light and maximum velocity of approach would obviously be approximately synchronous, and their coincidence would naturally be independent of the direction of the observer in space.' And in a suggestive foot-note Shapley remarked that on the pulsation theory the long-period variables might also find their explanation. 'Are they', he asked, 'merely Cepheids of very long period? In many respects they appear to be—the light-curves are similar, the shape and time of successive maxima often oscillate around probably rigorously constant mean values, the spectra change progressively throughout the light-period, and finally they are apparently stars of great absolute luminosity.'

Since 1914 much information has been collected which tends to confirm the pulsation theory. The Cepheids are giant stars, and this explanation of them as vast inflated masses is in harmony with Russell's theory of stellar evolution, while Eddington's theoretical researches on stellar radiation have confirmed the pulsation hypothesis. Cepheid variation is apparently a stage—perhaps a necessary one—in stellar evolution. 'The concurrent progression', says Shapley, 'towards a definite limit of luminosity, spectrum, and period suggests that in the evolutionary sequence of stars Cepheid variation is abruptly limited at or near the blue end of the giant series because of the changing physical conditions in the interior of the gaseous masses.' 'Apparently typical variation of this kind can affect only giant stars, and these only so long as they are not too condensed to obey the ordinary gas laws.'

It appears to be within the bounds of possibility that a number of stars which have been announced and catalogued as spectroscopic binaries are actually pulsating single stars. In a recent paper on the *Origin of Binary Stars*, Dr. J. H. Jeans of Cambridge expresses doubts as to whether the four stars, Rigel, Polaris, Capella, and Alpha Persei are actually binaries.

Almost all giant spectroscopic binaries, he says, ‘ show the general characteristics of Cepheid variation ’, and ‘ are most likely not binaries at all ’. Certainly the pulsation theory of Cepheid variation has had far-reaching results. One of these is the idea that stellar variability is not, as was formerly believed, an abnormal state, but a phase of development organic to the giant stage of stellar existence. It may indeed be that in the solar cycle we have a legacy from the turbulent giant stage of the Sun’s life.

VIII

NEW STARS AND NEBULAE

It is recorded that the formation of the first catalogue of stars ever compiled was undertaken by Hipparchus in 134 B. C., because of the appearance of a new star in the sky. Since his day, many new stars, or rather temporary stars or ‘ novae ’, to give them their more technical name, have shone out for a time and have then faded and died out. The brightest nova of modern times was observed by Tycho Brahe in Cassiopeia in 1572 ; it was estimated by him to equal Venus in brilliance. Tycho’s star was nearly equalled at maximum by the nova associated with the name of his disciple, Kepler. This star, which shone out in the constellation Ophiuchus in 1604, was estimated by Kepler to be the equal of Jupiter. In the seventeenth century only one or two novae were recorded, and in the eighteenth none at all. Widespread interest therefore attached to the ‘ blaze star ’ which shone out in the Corona Borealis in 1866. This star, which attained within a few hours the brilliance of a star of the second magnitude, was discovered by an Irish amateur, John Birmingham, and was observed by Huggins by means of the spectroscope immediately after its discovery. The chief feature of the spectrum was the presence of the bright lines of hydrogen gas.

Bright lines indicating the presence of glowing gas in great quantities have characterized the temporary stars seen since 1866. Of these the chief have been Nova Cygni of 1876, discovered by Schmidt ; Nova Aurigae of 1892, Nova Persei of 1901, which attained the first magnitude, rivalling Capella in brilliance ; Nova Geminorum of 1903, Nova Aquilae of 1918, which exceeded Nova Persei, and was in point of fact the brightest nova seen since Kepler's star shone out in 1604 ; Nova Cygni of 1920, and Nova Pictoris of 1925. These were among the brightest of a large number of temporary stars.

In 1898 Young remarked that ' it now seems very probable that the new stars would not be very uncommon if the small stars could all be closely watched '. This surmise has been confirmed, as the result of the persistent charting of the sky by means of photography. The appearance of novae is evidently an everyday occurrence ; and as instrumental power increases the number of recorded novae will in all likelihood increase at a correspondingly rapid rate.

Since the appearance of the ' blaze star ' of 1866, numerous theories have been advanced to explain the spectroscopic observations. These may be grouped into three classes—binary star theories, collision theories, and ' star-and-nebula ' theories. In 1890 Wilsing of Potsdam made an effort to apply to new stars the hypothesis of Klinkerfues, that stellar variability may be due to the mutual attraction of close binaries. According to Wilsing, temporary stars may be simply binaries with highly eccentric orbits and very long periods. Powerful tidal effects, on this theory, take place during the close approach of the two bodies, and as a result there is not only a great deformation of the stellar atmospheres, but also an outrush of incandescent vapour from the interiors and a great increase of light. The hypothesis was designed to explain the various kinds of spectra characteristic of temporary stars. For instance, the spectrum of Nova Andromedae of 1885 was continuous, while Nova Coronae and Nova Cygni exhibited the usual bright lines. In the first case, on this theory, the tidal effects were not strong enough to affect the interiors of the stars. On the whole, we may truly say with Scheiner that ‘ there is something improbable in the assumption of binaries with periods of many hundred years and yet with such exceeding slight periastron distances ’. A similar criticism may be brought against Lockyer’s idea that temporary stars are produced by the clash of binary meteor swarms. That theory was, of course, an integral part of the meteoritic hypothesis, and has been generally rejected.

The collision theory was advanced in 1889 by Croll in his work on stellar evolution, as a component part of his pre-nebular hypothesis. The idea was accepted by Huggins in his theory of Nova Aurigae. Instead of postulating a direct collision, Huggins adopted the general standpoint of Wilsing, ‘ that under such circumstances of near approach enormous disturbances of a tidal nature would be set up ’, but he believed that ‘ the great relative velocity of the component stars of the nova seems to point rather to the casual near approach of bodies possessing previously considerable motion ’. The idea of the close approach of two bodies was also accepted by Lowell and by Chamberlin and Moulton in their theory of the origin of nebulae. This general theory was modified by Vogel to explain the phenomena of Nova Aurigae. According to Vogel that outbreak was due to the entrance into a planetary system of a dark star travelling with an enormous velocity. Direct collisions would result between the stranger and some of the planets, while the smaller planets would be more or less reduced to vapour. Vogel sought by this theory to explain the many changes in the spectra, the presence of bright and dark lines, and the varying velocities ; while the temporary revival of the star was regarded as due to the meeting of the wanderer with an outlying planet of the system. Many serious objections were raised to Vogel's theory, and it gained little support.

The 'star-and-nebula' theory, originally suggested by Monck, is definitely associated with the name of Seeliger, who developed it and applied it to Nova Aurigae. It has since been further developed by Halm. According to this hypothesis, temporary stars are due to the passage of dark or feebly luminous bodies through masses of nebulous matter. On entering a nebula a dark body is more or less raised to incandescence. The result will be the production of two superposed spectra, that of the star being continuous, while the other spectrum will consist of bright lines. The revival of Nova Aurigae was explained by Seeliger as due to the entry of the star into an adjoining mass of nebulous matter. Scheiner objected that the high relative velocity indicated by the displacements of the lines was not accounted for by Seeliger. It is doubtful, however, if his objection is valid in the light of later knowledge of other possible causes of spectral displacements, such as pressure.

As long ago as 1876 it was ascertained that the spectrum of Nova Cygni had become predominantly nebular. This development of the nebular line in the spectra of novae has been proved to be characteristic of all these objects. The connexion of novae with nebulae was demonstrated by the discovery of nebulous matter around Nova Persei in 1901. But the nebular stage seems to be only a transient one. In 1880 the spectrum of Nova Cygni was observed at Harvard to be purely stellar; in 1903 Perrine made a similar observation on Nova Aurigae. In 1907 Hartmann found the spectrum of Nova Persei to have lost its nebular character completely, and to be essentially identical with that of the Wolf-Rayet star, B.D. +35° 4001. Some years ago the spectra of four temporary stars were closely studied at Mount Wilson by Adams and Pease. By July 1914 they discovered that the two oldest, in Auriga and Perseus, had become Wolf-Rayet stars, and by April 1915 the other two had become similarly transformed. In connexion with these discoveries, Adams and Pease made the suggestive remark that 'this identity of spectrum, taken in connexion with the well-known agreement of distribution relative to the Milky Way, of novae and Wolf-Rayet stars, makes it probable that at least a portion of the latter are temporary stars in the later stages of their history'.

In 1656 Huyghens drew attention to 'one phenomenon among the fixed stars worthy of mention', which, so far as his knowledge went, had 'hitherto been noticed by no one'. 'In the sword of Orion are three stars quite close together. In 1656, as I chanced to be viewing the middle one of these with the telescope, instead of a single star, twelve showed themselves (a not uncommon circumstance). Three of these almost touched each other, and with four others shone through a nebula, so that the space around them seemed far brighter than the rest of the heavens, which was entirely clear and appeared quite black, the effect being that of an opening in the sky through which a brighter region was visible.'

In the early years of telescopic astronomy the Latin term 'nebula' was applied to two classes of objects which are now known to be widely different in kind. Clusters of stars beyond the resolving power of the feeble instruments of the seventeenth and eighteenth centuries presented to the astronomers of the time a hazy, cloudlike appearance, and they were grouped with other misty objects such as the great nebulae in Orion and Andromeda. Nebulae of both types were classed together indiscriminately in the catalogue prepared by the French astronomer Messier in 1771 and revised by him in 1782. Messier's chief astronomical interest was the discovery of comets, and he drew up his catalogue of nebulae, which closely resemble telescopic comets in appearance, in order that he might not be deceived into the belief that he had found a new comet when he had merely re-observed an already known nebula. In his first catalogue Messier included 68 entries, while in his second there were in all 103, including the most familiar star-clusters and nebulae in the heavens.

Messier did not concern himself with the physical condition of the objects thus catalogued; but his catalogue happened to attract the attention of William Herschel, then at the commencement of his great quest after a knowledge of ‘ the construction of the heavens ’. Herschel thereupon undertook, with his improved telescopic means, a systematic examination of the objects noted by the French observer, and, in his own words, ‘ saw with the greatest pleasure that most of the nebulae which I had an opportunity of examining in proper situations yielded to the force of my light and power and were resolved into stars ’. Having completed this survey, Herschel began to search for new nebulae. He communicated to the Royal Society in 1786 his first catalogue of 1,000 new nebulae and clusters, and three years later a second catalogue with an equal number of entries. These were followed in 1802 by a list of 500 ‘ new nebulae, nebulous stars, planetary nebulae, and clusters of stars ’. In the discovery and observation of clusters and nebulae, Herschel's successor was his son, John Herschel, whose *General Catalogue*, published in 1864, comprised 5,079 nebulae and clusters.

At first Herschel believed that there was no essential difference between clusters and nebulae—that the irresolvable nebulae were simply star-clusters situated at vast distances in space, in which the individual stars could not be discerned. In fact, in his first paper on the construction of the heavens, he put forward the view that the stellar system was simply a nebula, ‘ our nebula ’, and that conversely the nebulae were island universes or independent galaxies of stars. Indeed, in 1785 he remarked that he had discovered ‘ fifteen hundred whole sidereal systems, some of which might well outvie our Milky Way in grandeur ’.

During his career Herschel was gradually led to the view that nebulae and star-clusters were totally distinct and that the great diffuse nebulosities and the planetary nebulae were composed of a ' shining fluid of a nature totally unknown to us '. On this idea he based his nebular hypothesis of stellar development, and during the remainder of his career he held firmly to the belief in the essential difference between star-clusters and nebulae. After his death opinion swung violently in the opposite direction. Increased telescopic power resulted in the resolution of a number of nebulae which had defied Herschel's great reflectors. Hence the erroneous conclusion was drawn that because some nebulae were resolvable therefore all would be resolved. Practically all the astronomers of front rank—including the younger Herschel himself—ceased to believe in the ' shining fluid ', and with it in the Herschelian concept of stellar evolution. The invention of the spectroscope, however, settled the question decisively in favour of Herschel's view. On 29 August 1864 Huggins turned his spectroscope on the bright planetary nebula in Draco, numbered 6,543 in the *New General Catalogue*. Great was the surprise in scientific circles at his announcement that the spectrum was one of bright lines—a conclusive proof of the objective existence of Herschel's ' shining fluid '. Later on, the Orion nebula and several bright irregular and planetary nebulae were found by Huggins to have gaseous spectra.

The reinforcement of the telescope and spectroscope by the photographic plate has led in the realm of nebular astronomy to results of far-reaching importance. In the last decade of the nineteenth century the late Dr. Isaac Roberts in England, Professors Keeler and Barnard in America, and Professor Max Wolf in Germany, began their photographic surveys of the heavens, which led to the detection of great numbers of previously unknown nebulous objects—vast diffuse nebulosities chiefly in the galactic regions, and great numbers of small nebulae outside of the galactic circle altogether. Keeler, with the Crossley reflector of the Lick Observatory, announced shortly before his death in 1900 that 120,000 new nebulae were within reach of that instrument, and that probably 60,000 were of spiral form. At the present time, it has been estimated, about five hundred thousand nebulae are known to astronomers.

Twentieth-century research with telescope, spectroscope, and camera has emphasized the division of the nebulae into several groups or classes which appear to be quite distinct. First of all, there are the bright irregular nebulae, of which that in Orion is the most prominent ; secondly, and closely connected with these, the dark nebulae ; thirdly, the planetary nebulae ; fourthly, the spirals ; and fifthly, a class of small

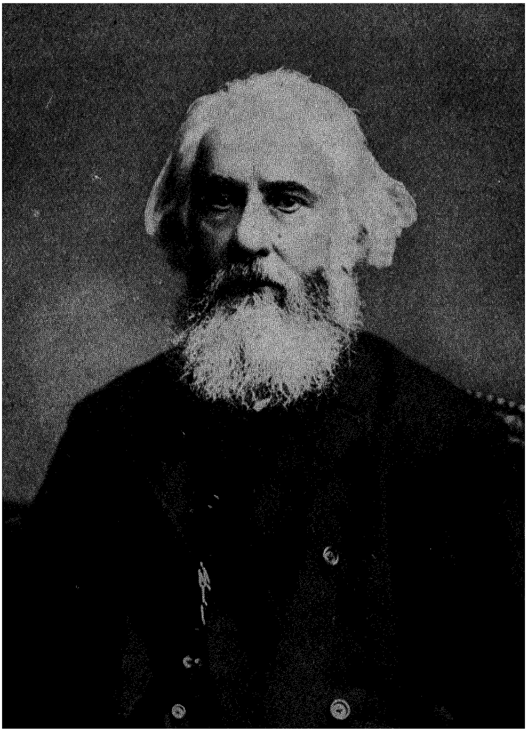

FIG. 20. WILLIAM HUGGINS

round objects, which were formerly classified with the spirals, but obviously differ from them in many important respects.

The discovery that the bright irregular nebulae are gaseous gave powerful support to the view that they represented ‘ worlds in the making ’, the material which would be, in the course of ages, wrought into suns and systems. This, indeed, appeared to be placed beyond all doubt by the association of these great nebulous masses with the so-called ‘ early-type ’ stars, especially with the helium and Orion classes. This association is one of the indisputable facts of stellar astronomy. As Campbell has concisely put it :

> ‘ Wherever there is a great nebulous region, either in or near or outside the Milky Way, you will find the class B and earlier types of stars abnormally plentiful ; and the chances are fairly strong that some of the stellar spectra will contain bright lines. . . . If you see a wisp of nebulosity near a bright star, look up the star’s spectrum, and you will probably find it an early class B, as in the case of Gamma Cassiopeiae, a second-magnitude star with nebulous structure near it, whose spectrum contains both dark and bright lines of hydrogen or helium. . . . If you find a red or yellow star of normal type, do not look for a nebula in apparent contact with it. Nebulae and red stars do not co-exist.’

In recent years, however, the aspect of the question has been completely altered by the formulation of Professor Russell’s theory of stellar evolution. According to this hypothesis, which has been confirmed both by theoretical investigation and observational evidence, the blue-white stars are not the youngest stars, but are suns in their prime, in the meridian of stellar life. If the blue stars are not the youngest of the suns, what, it may be asked, is the nature and function of the diffused gaseous nebulosity ?

The question has been still further complicated by one of the most remarkable discoveries of recent years—that of the second class of nebular objects, the dark nebulae. Herschel drew attention over a century ago to several dark spots in the galactic regions, which he referred to as ‘ holes in the heavens ', and his son detected a number of others. During the last century it was believed that these dark spots were simply regions free from stars, and it is probable that a number of them do represent vacant spaces. In 1902, however, Professor Max Wolf remarked on their symmetrical arrangement in regard to the stars and visible nebulae in their vicinity. He drew attention to a bright nebula in Cygnus, ' placed centrally in a very fine lacuna, void of faint stars, which surrounds the luminous cloud like a trench. The most striking feature with regard to this object is that the star-void halo encircling the nebula forms the end of a long channel, running eastward from the western nebulous clouds and their lacunae to a length of more than two degrees ' ; and he asked the question, ' Is there a darkness following the path of the nebula, absorbing the light of the fainter stars ? ' Dr. Wolf was gradually led to the opinion that these dark objects were not vacant spaces, but were actually composed of dark gaseous matter.

Independently of Dr. Wolf, the late Professor Barnard was impressed about the same time by the vacant regions of the sky, which, he predicted in December 1905, ' will before long excite as much study and attention as the nebulae '. At that time Barnard believed that the great majority of these dark areas were simply spaces void of stars, which many of them undoubtedly are ; but he noted certain regions where this explanation did not appear to be quite satisfactory. In regard to a nebulous region in Scorpio he wrote : ' The blending of this great nebula into the surrounding region where it seems to mingle with the material of the vacancies makes it hard to tell where the nebula leaves off. . . . There is a slight suspicion that certain outlying whirls of this nebulosity have become dark, and that they are the cause of the obliteration of the small stars near.' These ' slight suspicions ' of 1905 became certainties within the next decade. Barnard became gradually convinced, from a study of his own photographs and also as the result of visual investigations of several of these areas, that many of them were not mere starless spaces, but were 'really obscuring bodies nearer to us than the distant stars. . . . Their apparent preference for the Milky Way is obviously due to the fact that they are more readily shown with a bright background. They are, however, not strictly confined to the Milky Way.' By January 1919 Barnard was able to publish a catalogue of 182 of these dark markings.

But dark nebulae have been discovered by their reflection as well as by their obscuration of starlight. Kapteyn's explanation of the apparent expansion of the nebula surrounding Nova Persei of 1901 was one of the earliest suggestions that dark nebulous matter actually existed, and that it was capable of reflecting the light of stars. But, after all, this particular dark nebula had been invoked as a hypothesis. To Dr. V. M. Slipher belongs the actual distinction of discovering dark nebulae shining by reflected light. He was not, it is true, the first to observe the nebula round Merope in the Pleiades, which had been familiar to astronomers since 1859. Before 1912, however, it had been assumed to be self-luminous. But in December of that year Dr. Slipher secured several spectrograms of the nebula with the 24-inch Lowell refractor, and he made the unexpected discovery that 'the nebula shines by light which is a true copy of that of the neighbouring star Merope and of the other bright stars of the Pleiades'. From this he drew the conclusion that the Pleiades nebula shines by reflected light. In 1916 he succeeded in obtaining spectrograms of another diffused nebulosity—that surrounding the star Rho Ophiuchi. 'It appears', he said, 'that the spectrum of this nebula is continuous, and as far as can be judged from this weak plate, it is like that of the star Rho Ophiuchi, about which the nebula clusters. . . . The indications are that this nebula is shining by reflected light, as was found to be true of the nebula in the Pleiades.' He added that 'in both these regions of the sky faint stars are conspicuously deficient in numbers. The type of spectrum conforms to the view that the scarcity of stars in these and certain other regions is due to light absorption by nebulae which may otherwise be invisible.'

An extensive and systematic study of the nebulae—their distribution and their spectra—has been carried through within the last few years by Dr. Edwin P. Hubble, of the Mount Wilson Observatory. This distinguished American astronomer has suggested a classification of the nebulae into galactic and non-galactic. The galactic includes the planetaries and the diffuse nebulae; and under this latter head Dr. Hubble groups both bright and dark. In his discussion of the non-galactic nebulae, Dr. Hubble strongly combats the contention that all non-galactic nebulae are spirals, which he characterizes as a 'daring extrapolation', 'not justified by our present knowledge of nebular forms'. He divides the non-galactic nebulae into four classes—spiral, elongated, globular, and irregular.

In his study of the distribution of the galactic nebulae, Hubble brings out the interesting fact that the large planetaries are distributed over the sky with some approach to uniformity, while the small ones are strongly concentrated to the plane of the Galaxy. Making the reasonable supposition that the apparent diameters and nuclear magnitudes may be taken as indications of distance, he concludes that 'when real distributions are determined, the vertical deviations from the galactic plane will prove to be relatively small'. In the case of the diffuse nebulae, their distribution is not a mere concentration along the galactic plane. These nebulae are concentrated towards two distinct belts—the galactic plane itself and a belt inclined to it by twenty degrees. This latter has approximately the same nodal points as the bright helium stars which form the outline of Dr. Shapley's 'local cluster'.[1] The meaning of this double concentration evidently is that some of the diffuse nebulae are concentrated in the local cluster and others in the Milky Way clouds. So it

[1] For discussion of the local cluster see next chapter.

would seem that conspicuous nebulae are practically non-existent in the vast spaces between.

But the most striking of Dr. Hubble's results is his discovery of the relation between the spectra of the diffuse nebulae and the involved stars. He finds that stars involved in nebulae having continuous spectra, and presumably shining by reflected light, are nearly always of spectral type B 1 or later; while stars involved in nebulae with emission spectra are practically always of earlier type than B 1, which is apparently a critical point in the spectral sequence. In the case of the planetary nebulae, Hubble finds that the central stars of small planetaries are of Wolf-Rayet type, while those of large planetaries are intermediate between the Wolf-Rayet type and the type Oe 5.[1] 'This intimate relation between spectral type of nebula and of involved stars', Hubble maintains, 'raises a presumption that one is a consequence of the other. It seems more reasonable to place the active agency in the relatively dense and hot stars than in the nebulosity.' Apparently, the emission nebulae are excited to luminosity by the radiation of the very hottest stars. The cooler stars are seemingly incapable of doing this, and the nebulous material which surrounds them shines by reflected light.

Dr. Hubble's investigation has brought out the fact that practically every diffuse nebulosity is near enough to a bright star or group of stars to be under stellar influence. Obviously, then, luminosity is not a necessary property of nebulae. The normal nebula is dark, and the bright nebula is but a particular case of the dark. Bright nebulae seem, in fact, to be merely portions of dark clouds of nebulous matter. Thus, in the case of the great Orion nebula, Russell maintains that 'there is no reason to believe that the luminous gas forms the whole or even any large part of the matter present within the region—only that it is selectively sensitive to the incident excitation, and therefore gives out most of the light, just as the gases (carbon compounds and nitrogen) do in the coma and tail of a comet '. The Orion nebula is simply a ' superficial fluorescence ' of a vast dark cloud.

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[1] The Harvard types of stellar spectra have been divided into various sub-types.

The question of the origin of these dark clouds has been investigated by Russell, and he finds a plausible explanation in radiation pressure. Dwarf stars are practically incapable of repelling dust-particles, but the giants, and especially the B stars, repel such particles very powerfully. All the finer particles are driven away from these hot and massive suns. ' This selective removal, from the vicinity of bright stars, of the particles which are most efficient in cloud formation, may explain the fact that the luminous portions of these dark nebulae, though centred upon stars, do not brighten up in their immediate neighbourhood as might have been anticipated.'

The great extended nebulae—bright and dark—appear to be almost at rest in space. This is in marked contrast to the planetary nebulae, whose average speed as determined by the spectroscope, is about forty-eight miles per second. These ' planetaries ', so named by their discoverer, Sir William Herschel, from their resemblance to planetary discs, form indeed a small and select class, sharply differentiated from the other types. Like the irregular and dark nebulosities, they congregate towards the galactic plane, and they are truly gaseous, as Herschel believed them to be. But their regular contour and rapid motion, as well as their comparatively small number—only about 150 are known—mark them off as a class apart. They are almost always distinguished by the presence of a central star, so that from one point of view these may be regarded as stellar rather than as nebular objects. They are closely related to the rare type of star known as Wolf-Rayet stars, which are characterized by bright lines in their spectra. ' Indeed ', remarked Dr. Wright, of the Lick Observatory, in 1914, ' there is no escape from the conclusion that the nuclei of planetary nebulae not only are closely related to Wolf-Rayet stars, but in many cases they are such stars.' The parallaxes of six planetary nebulae have been measured by Mr. Van Maanen, of Mount Wilson, within the last few years, and he finds the mean absolute magnitude of these stars to be +9·1, indicating them to be dwarfs.

Dr. Curtis, of the Lick Observatory, has suggested that the planetaries are to be regarded as 'a somewhat sporadic case in stellar evolution, arising through some collision or cataclysm and not to be regarded as cases typical of the general run of stellar development'. Considerable support is to be found for this view—not only in the rarity of the planetaries and their enormous velocities, but also in the connexion already mentioned between such nebulae and temporary stars. On the theory of Seeliger, as already mentioned, these novae are caused by the rush of feebly luminous stars through masses of invisible nebulosity, and the nebular and Wolf-Rayet stages may be successive stages in the passage and emergence of a star into and from such a nebula. Be this as it may, there is much to be said for the suggestion that the planetary nebulae are 'the wrecks of ancient novae'.

The discovery of the spiral nebulae was the outstanding achievement of Lord Rosse's great 6-foot mirror. Fourteen nebulous objects appeared to Lord Rosse and his assistants to be of a spiral structure; of these the greatest was the nebula catalogued by Messier as 51 in the constellation Canes Venatici. The announcement of the discovery was received with considerable incredulity, and it was not until the advent of photography that the spiral structure was placed beyond doubt. Isaac Roberts in England and Keeler at the Lick Observatory not only confirmed Lord Rosse's observations of the fourteen particular objects, but discovered many new spirals all over the sky. Shortly before his death, in 1900, Keeler announced that there were 120,000 new nebulae within reach of the Crossley reflector of the Lick Observatory, and that at least half of these were spirals. Later work has shown that the Andromeda nebula is really the greatest of the spirals, but its spiral form was not recognized until after the application of photography.

Three theories of the nature of these enigmatical nebulae have been before the scientific world. These are: (1) that the spirals are true nebulae, in process of development into stellar systems; (2) that they are formed of cosmic dust driven off from the stars; and (3) that they are external universes. About twenty years ago, after Keeler's announcement concerning the profusion of these objects, the prevalent view was that in the spirals, suns and systems were to be seen forming under the eye of the observer. Indeed, the famous planetismal hypothesis of evolution was based on the assumed gaseous nature of the spiral nebulae. About a decade ago, however, opinion veered round to the view that the spirals were not true nebulae at all, but irresolvable clusters at vast distances from our galactic system. In 1911 Professor F. W. Very revived the 'island universe' theory, which derived considerable support from the numerous differences between the spirals and the other members of the nebular family. In 1912 Dr. V. M. Slipher announced that the average velocity of the spirals is 'about twenty-five times the average stellar velocity', and Dr. Shapley has remarked that as a class—'apparently regardless of the gravitational attraction of the galactic system'—they are moving away from the stellar system. Further, the discovery in 1917, on photographs of spiral nebulae, of a number of faint temporary stars, gave plausibility to the idea that such might be the novae of distant galactic systems.

Thus the available evidence about the middle of the second decade of the century seemed to point to the view that the spirals were not true nebulae at all, but external universes which are too far away for the component stars to be separately discerned. This view seemed, about eight or ten years ago, to be likely to prevail; but more recently the work of Van Maanen, Lampland, Shapley, and others threw grave doubts upon its validity. Dr. Van Maanen and Mr. Lampland succeeded in measuring the motions of internal points in the nebula Messier 101. Dr. Shapley truly remarked in 1919 that these measures 'would indicate rotational velocities greater than the velocity of light, if that spiral is held to be even one-fifth as large as our galaxy now appears to be'. Finally, Dr. Shapley pointed out that 'if in real dimensions spiral nebulae were analogous to our galactic system, the absolute magnitude of the novae in spirals would far transcend any luminosity with which we are acquainted, and would be at direct variance with present results on intrinsic stellar brightness'. Some years ago Shapley suggested that 'the spirals represent the failure to form stars from the original condensing nebulosity through the presence of too much material'. On this hypothesis the spirals are composed of the unused material driven away towards the poles of the galaxy by the radiation-pressure of the stars, and would be, in a sense, analogous to the cometic and meteoric matter in the solar system. This is not out of harmony with Dr. See's idea that 'if repulsive forces are everywhere at work expelling dust from the stars for the formation of nebulae, it is evident that as it is repelled by the stars it will tend to gather, especially in vacant regions or spaces remote from the stars, and should accumulate with maximum density near the poles of the Milky Way'. On Dr. See's view, however, the nebulae are not waste material, but true nebulae, the progenitors of the suns and systems of the future.

In 1923 Professor F. A. Lindemann further developed these ideas in an elaborate hypothesis. According to this theory the spirals consist of dust-particles expelled from the stars by radiation-pressure. 'There must be a continuous stream of fine dust leaving the neighbourhood of the stars and moving towards regions of low radiation density.' On this hypothesis the very high velocities of the spirals are accounted for as a consequence of light pressure which may rise to eight times the gravitational attraction in the case of the Sun, while for stars radiating more per unit it may be considerably greater. Dr. Lindemann further suggested that the light of the spirals is not inherent, but due to reflection from the light of the stellar system as a whole. His explanation of the appearance of novae in the spirals is the weakest part of his theory. These outbursts, he suggested, are due to encounters between spiral nebulae and ‘ comet-like clouds of stones or sand ’—an idea which he admitted to be ‘ somewhat artificial ’.

Meanwhile, Dr. Jeans, the distinguished English cosmogonist, has held to the strictly nebular theory of the spirals according to which the spiral is the normal nebula out of which will develop the stars and systems of the future. These enormous nebulae, he has maintained, will develop not into solar systems, but into streams of stars. Indeed, he has contended that the process of evolution is actually taking place before our eyes, and that its rate can be calculated by the internal motions which Van Maanen and Lampland have claimed to have measured. In the case of the spiral M 101, believed by Van Maanen to be 5,000 light-years away, Jeans stated that ‘ this mass is, under the influence of increasing rotation, breaking up into streams of stars. Each star is of mass comparable with our Sun, and the stars are generated at the rate of one every few hundred years.’ Dr. Shapley pointed out in 1924 several important objections to these views. Observational evidence shows that ‘ spiral nebulae are not directly generating either stellar clusters or typical stars that are larger and less dense than the Sun ’. Were this theory correct, there should be many giant stars in the vicinity of spirals. Shapley finds that such is not the case.

It is evident that the validity of Dr. Jeans’ views of the spirals depends ultimately on the actual distances of these objects. If Dr. Van Maanen’s estimates of distance be confirmed and the spirals turn out to be as near or nearer to us than the star-clusters, then the nebular theory of their nature is almost certainly the true one. Early in 1925, however, Dr. Hubble announced that with the 100-inch Mount Wilson reflector he had succeeded in resolving partially two of the spirals—M 31 (the great Andromeda nebula) and M 33—into stars. Further, he detected Cepheid variables in both of these nebulae. Now, these variables have been used by Dr. Shapley to measure the distances of star-clouds and of the outlying star-clusters. By means of these, Hubble found for both these spirals a distance of about a million light-years. Jeans' theory would appear to be quite untenable—a fact which he himself seems to recognize. These nebulae are evidently outlying stellar systems.

Dr. Lundmark, of Upsala, has not only confirmed these measurements independently of Dr. Hubble, but he has removed several of the chief objections to the view that the spirals are distant stellar systems. The chief objection advanced by Shapley in 1919 was that if the new stars in the spirals are really novae, then their absolute magnitudes must considerably exceed the absolute magnitudes of the novae in our stellar system. But Lundmark has pointed out that the absolute magnitudes of the novae in our stellar system are higher than has hitherto been believed. 'Novae at their maximum', he finds, 'are among the brightest stars known in the Galaxy.' As a result of his work with the Crossley reflector of the Lick Observatory, Lundmark has concluded that in the case of M 33 'the so-called nebula is composed of stars, clusters of stars, and also of nebulosity in more or less the same way as our Milky Way system'. In his most recent paper Dr. Jeans seems to accept a similar view.

The alleged extraordinary profusion of the spirals has for long constituted a formidable problem on any hypothesis of their nature. They have been assumed to number from eight hundred thousand to a million. In 1919 the English astronomer Mr. J. H. Reynolds remarked that many nebulae classed as spirals are really small round nebulae which 'probably actually form the majority of the known nebulae'.

Confirmatory evidence of this has been obtained from Mount Wilson. Indeed, in a recent annual report of that observatory it was stated that ‘ the non-galactic objects which had been designated as globular nebulae are more numerous than those of any other class ’. Whether these globular nebulae actually form a class distinct from the spirals is still uncertain.

IX

COSMOLOGY

‘ A KNOWLEDGE of the construction of the heavens ’, wrote Herschel in 1811, ‘ has always been the ultimate object of my observations.’ In the last analysis this is the ultimate object of all scientific activity ; and so we find that from the earliest dawn of science downwards men have persistently sought a cosmology, a view of the world in its completeness. The speculations of the Ionian philosophers, and the observations of the empirical astronomers, which found expression in the Eudoxian and Ptolemaic theories of the world, were alike directed towards the same problem.

In medieval times, it is true, the general order of nature was believed to be fairly well known. The Earth was the hub of the Universe, fixed and immovable in the centre of creation. Round the Earth, at various distances and in different periods, revolved the Sun and Moon and the five planets or wandering stars, while beyond was the sphere of the ‘ fixed ’ stars. But this simple and easily comprehended view of the universe had to be abandoned when in the early seventeenth century the truth of the Copernican system was demonstrated by the discoveries made by means of the newly-invented telescope. The Earth was perceived to be merely one—and by no means the largest—of several planetary worlds revolving round the Sun ; so far from being the centre of creation, our world was seen to occupy a very humble place in the scheme of things.

At first the attention of astronomers was practically monopolized by the Solar System. The 'fixed stars' were studied incidentally; indeed, they were observed chiefly from the standpoint of fundamental astronomy, as convenient reference points for the determination of lunar and planetary positions and motions. As to the arrangement of the greater stellar system, nothing was known. About the middle of the eighteenth century, Thomas Wright, of Durham, put forward wonderfully sagacious speculations, in which he foreshadowed the disc-theory, and further acute guesses were made by Kant in 1755 and by Lambert in 1760. These speculations, however, were not based on observation, and the wonder is that in so many respects they were so near to the truth.

At the very beginning of his career Herschel saw clearly that the first desideratum was the accumulation of data on which a rational hypothesis of the structure of the stellar universe could be based. In dealing with the problem there are three lines of attack, through the study of the distribution, motions, and distances of the stars. Herschel's efforts to measure parallax proved abortive, and that avenue was closed to him. His knowledge of the proper motions of the stars was much too slender for him to build a cosmological concept thereon. His brilliant discovery of the solar motion was but a by-product of his wider research. He was therefore closed up to the one method of attack—the study of the apparent distribution of the stars.

In 1784, in his preliminary paper on the construction of the heavens, he described his method of star-gauging, which, he said, 'consists in repeatedly taking the number of stars in ten fields of my reflector, very near each other, and, by adding their sums and cutting off one decimal on the right, a mean of the contents of the heavens in all the parts which are thus gauged is obtained'. These gauges revealed wide differences in the star-density in various parts of the heavens. In the most crowded parts of the Galaxy, Herschel occasionally counted as many as 588 stars in a field of view, while in other parts of the sky there were fields practically vacant. Making two important assumptions, Herschel was led by these gauges to formulate a definite hypothesis of the sidereal system. He assumed that his gauging telescope was capable of penetrating to the boundaries of the system, and that throughout the system the stars were scattered with some approach to uniformity. Having made these assumptions, Herschel was able, in his second paper, dated 1 January 1785, to express himself thus :

> ' That the Milky Way is a most extensive stratum of stars of various sizes admits no longer of the least doubt, and that our Sun is actually one of the heavenly bodies belonging to it is evident. I have now viewed and gauged this shining zone in almost every direction, and find it composed of stars whose number, by the account of these gauges, constantly increases and decreases in proportion to its apparent brightness to the naked eye.'

Herschel was thus in a position to outline the general shape of the sidereal system, and to make a rough estimate of the scale on which it was constructed. He sketched it as a thin cloven disc of irregular outline, the cleft representing the well-known division in the Milky Way. In this system, which he believed to extend much farther in the galactic plane than in the direction of the poles, he believed the Sun to be placed near, but not quite at the centre—75 of Herschel's ' astronomical units '[1] from the north galactic pole and 80 from the south, and 597 units from the boundary of the system in the direction of Aquila, and 352 from the boundary in the direction of Canis Major. An astronomical unit Herschel took to be the computed mean distance of stars of the first magnitude. The galactic system he believed to be strictly limited in extent, and to be, in fact, only one system among others—an ‘ island universe ’. ‘ Our nebula ’, he maintained, ‘ is a very extensive branching compound congeries of many millions of stars.’

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[1] This term has now a different meaning. It is used to designate the Earth's distance from the Sun.

The name, ‘ our nebula ’, was suggestive. So great had been his success in resolving into separate stars the milky spots catalogued by Messier in 1783, that he confidently believed all nebulae to be clusters of stars which would yet be resolved by higher telescopic powers. All these nebulae he considered to be ‘ island universes ’. In 1785 he announced, indeed, that he had discovered ‘ fifteen hundred whole sidereal systems, some of which might well outvie our Milky Way in grandeur ’. He divided these nebulae, or universes, into four classes or ‘ forms ’—the forms differing in the degree of condensation or clustering. ‘ We inhabit ’, he said, ‘ the planet of a star belonging to a compound nebula of the third form.’

The original disc-theory, with its corollary, the hypothesis of island universes, was expounded for many years in text-books of astronomy and manuals of popular science as if it had been held by Herschel throughout his life in the exact form in which it was propounded. As Proctor truly remarked, ‘ it seems to have been supposed that his papers could be treated as we might treat such a work as Sir J. Herschel's *Outlines of Astronomy*—that extracts might be made from any parts of any paper without reference to the position which the paper chanced to occupy in the entire series ’. There can be no doubt that, as Struve and Proctor maintained after their careful study of Herschel's series of papers, the great astronomer altered his opinions. The wonder rather is that for half a century there was widespread ignorance of the fact, for Herschel himself remarked in his paper of 1811 : ‘ I must freely confess that by continuing my sweeps of the heavens, my opinion of the arrangement of the stars and their magnitudes and of some other particulars has undergone a gradual change.’

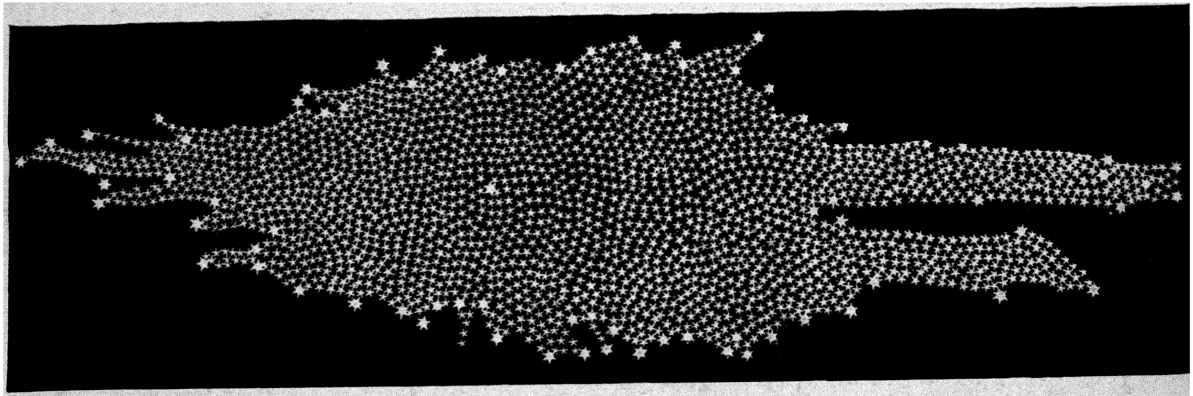

Fig. 21. THE STELLAR SYSTEM ACCORDING TO HERSCHEL'S DISC-THEORY

He was led to a modification of his views along two lines of research. In his first papers he had assumed an approximately equal scattering of stars, though he was quite clearly of opinion that the assumption was an approximation only. In his paper of 1785 he admitted that 'in all probability there may not be two or three of them in the heavens, whose mutual distance shall be equal to that of any other two given stars, but it should be considered that when we take all the stars collectively there will be a mean distance which may be assumed as the general one'. Even in the same paper Herschel remarked that it would not be difficult to point to two or three hundred 'gathering clusters' in the stellar system. Accordingly, he began to foresee as a result of what he called 'the clustering power' the breaking up of our galactic system into many small independent nebulae. From this it was but a step to his recognition in 1802 of the fact that 'this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments.' Herschel was thus forced to abandon his general view of the sidereal system as a collection of myriads of 'insulated stars', and to substitute for this the conception of a system consisting of many local groups and clusters.

His persistent study of the various classes of nebulae resulted in a change in his view of these objects and in the virtual abandonment of the 'island universe' theory. He was led by gradual stages to question his earlier generalization that nebulae were simply distant clusters; but it was in his paper on 'nebulous stars properly so-called' that he announced his belief in the existence surrounding such stars of a 'shining fluid of a nature totally unknown to us'. From this he came to reject the stellar theory of nebulae, first in the case of the planetary nebulae, then in that of the diffused nebulosities, such as that in Orion. The new view of nebulae as gaseous masses led to a revised estimate of their distances.

They came to be regarded as part of the general galactic system, and the theory of island universes fell into the background.

Did Herschel abandon the disc-theory, as Proctor and other acute thinkers have maintained? If by the disc-theory we mean the view of the stellar system as composed of more or less evenly-distributed 'insulated' stars, the answer is in the affirmative; for in the course of his investigations Herschel came to recognize not only the existence of binary stars, but of local groups and aggregations, especially in the galactic plane. But if by the disc-theory we mean the concept of the stellar system as having a much greater extension along its plane than in the direction of its poles, then Herschel cannot be said to have altered his views. In 1817 and 1818 he communicated two papers to the Royal Society on the extent of the Milky Way, and on the relative distances of star-clusters. In these papers he outlined his new method of star-gauging, which has been confused by numerous writers with the first. The two methods, however, were essentially distinct. In the first, one telescope was used on different areas of the sky; in the second, on the other hand, the same region was examined by different instruments. Once again Herschel assumed for the sake of investigation a certain 'properly modified equality of scattering', and a certain equality of real brightness. In the paper of 1817 he applied this method to the Milky Way, and in that of 1818 to clusters, assuming that the relative distances of globular and other clusters can be determined by the telescopic powers necessary to reveal and resolve them, and also that the component stars are, generally speaking, comparable to Sirius in size. His ultimate conclusion was that the sidereal system was considerably more extended in the plane of the Galaxy than he had previously believed. 'The utmost stretch of the space-penetrating power of the 20-foot telescope', he wrote in 1818, 'could not fathom the profundity of the Milky Way; and the stars beyond its reach must have been farther from us than the 900th order of distance.' And in 1818 his final conclusion was that when our gauges will no longer resolve the Milky Way into stars, it is 'not because its nature is ambiguous'—possibly nebulous—'but because it is fathomless'.

It was partially on these words that the elder Struve based his hypothesis of a sidereal system infinitely extended in the galactic plane. Struve's own study of stellar distribution led to the formulation in 1847 of his modification of the original disc-theory. The universe was, he believed, of finite thickness—the stars being obviously limited in number in the direction of the galactic poles—but of infinite extension in the direction of the Galaxy. He maintained, indeed, that this was borne out by Herschel's later investigations, and that when Herschel spoke of the Galaxy as fathomless he meant that it was unfathomable. Along with this view of an infinite extension in the galactic plane, Struve advanced his theory of the extinction of light in space—maintaining that the more distant of the galactic star-clouds were rendered invisible by the extinction of light. Struve's hypothesis was rejected by the majority of his contemporaries, and their rejection has been justified by subsequent research. Grant and the younger Herschel showed that in important respects it failed to explain the observed facts; while Encke demonstrated that the theory was based on five assumptions, all of which were questionable.

The work of Proctor in England may be regarded as analogous to that of Struve on the Continent. He made an exhaustive study of Herschel's papers, and proved beyond a doubt that the detailed disc-theory of 1785, with its corollary—the hypothesis of island universes—had been definitely abandoned by Herschel. His own views of the universe represented a reaction from the views of Struve; he regarded the universe as much less extended in the plane of the Galaxy than Struve, or even Herschel, had believed.

'Where', he wrote, 'Herschel thought he was penetrating to

> the extreme limits of the sidereal system, he was in reality only analysing more and more searchingly an aggregation in which many orders of stars were mixed up. What he failed to do was not, as he supposed, to sound the Galaxy, but to recognize as separate stars the minutest order of orbs included within such aggregations.'

Proctor believed the stars composing the Milky Way to be smaller and more closely crowded together than those in the vicinity of the Sun. In his view, therefore, the extent of the sidereal system was much less than Herschel had believed it to be, although he admitted that we 'are still bound to accept a considerable extension of our system in the galactic plane as fairly deducible from the gauges of Sir W. Herschel'.

In 1870 Proctor plotted on a single chart all the stars, to the number of 324,198, contained in Argelander's *Durchmusterung*. This chart revealed the aggregations of the brighter stars towards the galactic plane—a discovery abundantly verified by the independent work, years afterwards, of Schiaparelli and Gore—and also the connexion between bright stars and the nebulous light of the galactic background. 'It is', he said, 'utterly impossible that excessively remote stars could seem to be clustered exactly where relatively near stars are richly spread.' The general conclusion which Proctor reached was that

> 'the sidereal system is altogether more complicated, altogether more varied in structure, than has hitherto been supposed. Within one and the same region coexist stars of many orders of real magnitude, the greatest being thousands of times larger than the least. All the nebulae hitherto discovered, whether gaseous or stellar, irregular, planetary, ring-formed, or elliptic, exist within the limits of the sidereal system. They all form part and parcel of that wonderful system whose nearer and brighter parts constitute the glories of our nocturnal heavens.'

The excess of bright stars on the Galaxy was interpreted by Proctor as proving that the bright and faint stars were comparatively close together, and in this he was followed by Gore. Celoria, however, interpreted the fact as indicating the existence of two galactic rings, while Gould, an able American astronomer, believed the 'secondary galaxy' of lucid stars to be due to the existence of a flattened cluster of bright stars comparatively close to the Sun. This suggestion, however, did not receive much attention, and was rejected by both Gore and Newcomb. The former astronomer, writing in 1898, regarding Kapteyn's revival of Gould's suggestion in another form, expressed the view that 'the Sun is a member of a cluster of stars, possibly distributed in the form of a ring, and that outside this ring, at a much greater distance from us than the stars of the solar cluster, lies a considerably richer ring-shaped cluster, the light of which, reduced to nebulosity by immensity of distance, produces the Milky Way gleam of our midnight skies'.

This conclusion, however, was not altogether supported by the exhaustive investigations on stellar distribution carried out by Seeliger, of Munich, from 1884 to 1898. Newcomb, writing in 1901, referred to this as 'the most thorough study of the distribution of the great mass of stars relative to the galactic plane'. Dealing with over 100,000 stars, Seeliger demonstrated the gradual increase of the number of stars from each of the galactic poles to the Milky Way. Now, if the Galaxy were simply a ring of stars surrounding a star-sphere, the number of stars would increase not gradually, but suddenly near the boundary of the ring. 'The Milky Way', said Seeliger, 'is no mere local phenomenon, but is closely connected with the entire constitution of our stellar system.' Seeliger, in thus emphasizing the fundamental importance of the galactic zone, was led nearer to the standpoint of the elder Herschel.

At the close of the century several estimates had been made of the possible extent of the stellar universe. When the original disc-theory was propounded by Herschel the actual distances of stars were unknown; but he assigned to the universe in its greatest extension a diameter 950 times the mean distance of stars of the first magnitude, the thickness of the disc, in the direction of the galactic poles, being 155 of these units. More definite estimates were possible after the measurements of stellar parallaxes had indicated the scale on which the universe is built. Newcomb, in 1901, estimated the distance of the stars at the boundary of the universe as 3,000 light-years; and, as the Sun was believed to occupy a fairly central position, this indicated a diameter of 6,000 light-years. Seeliger, on the other hand, placed the limits of the universe at 9,000 light-years from the Sun, the diameter being about 18,000 light-years. More moderate estimates were adopted by other competent authorities.

The problem of the construction of the universe, as has been mentioned, may be attacked from three different standpoints—stellar distribution, motion, and distance. The earlier astronomers were forced to concentrate on ascertaining the facts of stellar distribution, and had to be content with deductions from these facts. Even down to the close of the nineteenth century the known motions and distances of the stars were too few, and in many cases uncertain, to warrant the formulation of a hypothesis based upon them. Mädler, it is true, made a bold attempt to locate the centre of the stellar system from a study of the available proper motions. His data were too scanty to permit even of partial success. But his work deserves to be remembered as the first attack on the problem through the avenue of proper motions. Mädler reasoned that the mutual attraction of the different stars would cause the bodies at the confines of the system to move more rapidly than those at the centre, and accordingly he sought the centre of gravity in a region of sluggish proper motion. He concluded that if a community of motion in a direction opposite to that of the Sun be found in any part of the heavens, it might be reasonably assumed that the centre of gravity of the stellar system lay in that region. He found in the constellation Taurus a common proper motion, small in amount, and he fixed on Alcyone, the chief star of the Pleiades, as the central Sun, round which he calculated the Sun moved in a period of about eighteen million years. Struve rejected Mädler's speculation as ' much too hazardous ', and this verdict was generally endorsed. The time was not ripe for generalizations of the kind. Over half a century was to elapse before a sufficient number of proper motions were known with some degree of accuracy to warrant a frontal attack on the problem by these means.

The latter half of the nineteenth century witnessed the accumulation of a vast mass of material relating to stellar positions. Schönfeld's extension of Argelander's *Durchmusterung* to the southern skies was the first of a number of important undertakings with a view to exhaustive acquaintance with stars of fainter and fainter magnitudes. The American astronomer Gould completed in 1879 a catalogue of 8,198 southern stars, and in 1886 a catalogue of 32,448. In 1882 Gill, at the Cape of Good Hope, applied photography for the first time to star-charting, and in the four years 1885–9 he undertook by means of photography the extension of the *Durchmusterung* from the point where Schönfeld left it to the southern pole. The reduction of the plates and formation of a catalogue was undertaken by Kapteyn, of Groningen, and the work was completed in 1900. By this time a greater undertaking was in progress. This was the *Astrographic Chart and Catalogue,* an international work assigned to eighteen observatories in all nations. The enormous mass of material accumulated by these surveys is, of course, somewhat unwieldy, and in 1906 Kapteyn proposed his ' plan of selected areas '. He chose 206 areas distributed uniformly over the sky, and proposed the construction for these areas of catalogues of magnitudes, proper motions, radial velocities, &c., down to the extreme limits of faintness attainable. Various observatories have co-operated in this highly important piece of work. Simultaneously, the Mount Wilson Observatory has carried through important counts of fainter stars, while at Harvard Observatory the *Henry Draper Catalogue of Stellar Spectra* had been brought to completion.

The great mass of material collected since Bradley's time bore fruit in the early years of the century, when Kapteyn found it possible to attack the problem of the structure of the universe by means of the study of known proper motions. In 1904 he read at the International Congress at St. Louis, U.S.A., a paper of far-reaching importance, in which he announced the discovery of star-streaming. As is well known, there are two components in the motion of each star: (1) the parallactic motion due to the movement of the Sun, carrying with it the Earth and the other planets, and (2) the individual proper motion. Formerly it was believed that after the parallactic motions were eliminated the individual motions were more or less at random. This assumption Kapteyn showed to be erroneous. After eliminating the parallactic motion from the motions of the bright stars in the catalogue of Bradley, revised by Auwers, Kapteyn found that the motions of the stars fell into two opposite directions in the galactic plane. This result he interpreted as indicating that the brighter and nearer stars belong to one or other of two vast interpenetrating streams.

Shortly afterwards these results were abundantly confirmed by the independent work of Professor A. S. Eddington, then chief assistant at Greenwich, now director of the Observatory at Cambridge. In his first paper on *The Systematic Motion of the Stars*, read before the Royal Astronomical Society in November 1906, Eddington made what he called 'the attempt to subject Kapteyn's theory to a quantitative test by examining the Greenwich-Groombridge proper motions. The following reasons, among others, seemed to make this desirable. The Bradley stars are all bright stars; of those of the Groombridge catalogue a large number fall between the seventh and ninth magnitudes. The 2,500 stars of Bradley's catalogue cover most of the sky; the 4,500 stars of Groombridge are all within 52° of the north pole.' The result of the investigation was a complete confirmation of Kapteyn's theory. Eddington ascertained that the velocity of the first drift of stars relative to the Sun was very much greater than that of the second, and that there was no appreciable difference in the main distances of stars of the two drifts. In a later paper Professor Eddington considered the motions of 5,322 of the stars of the *General Catalogue* of the American astronomer, Dr. Lewis Boss. Again, he found that the proper motions fell into two separate drifts, with the relative motion of the two drifts parallel to the galactic plane.

A further stage was reached by the paper which Sir F. W. Dyson, then Astronomer Royal for Scotland, read before the Royal Society of Edinburgh in 1908. His investigation covered 157 stars within 52° of the north pole, taken from Groombridge's catalogue; 165 stars within 50° of the south pole from the Cape *Astrographic Catalogue*, from Porter's *Cincinnati Catalogue*, and the *Cape General Catalogue*; and 778 stars within a belt extending 30° north and south of the equator, from the *Cincinnati Catalogue*. The results of this study of stars of very large proper motion, scattered all over the sky, were strongly in support of the two-drift hypothesis. 'The quick-moving stars here considered show the streams in a very pronounced light.' Confirmation of the two-stream theory was also forthcoming from Professor Campbell's investigation of radial motion. Dr. Halm, of the Cape Observatory, not only confirmed the hypothesis by his own researches, but came to the conclusion that there were not only two drifts, but three.

Various hypotheses were put forward to explain the existence of the two star-streams. Local streams had, of course, been known since Proctor pointed out the community of proper motion of five of the stars of the Plough; but the work of Kapteyn, Eddington, and Dyson indicated that the great mass of the brighter stars belonged to two or three streams, and this called for a theoretical explanation. The problem before the theoretical astronomer was to preserve the unity of the universe. In 1908 Schwarzschild, of Potsdam, put forward his ellipsoidal hypothesis, and in 1912 Turner, of Oxford, outlined a new theory reconciling the observed duality of the universe with the unity which theory seems to demand. In 1911 Eddington remarked, however, that ‘ the two star-streams probably involve at least half a million of the stars around us ; but there has never been any evidence that they prevail in the extremely remote parts ’. This has been confirmed in recent years, and the two-drift phenomenon finds its explanation in the comprehensive cosmological scheme of Dr. Harlow Shapley.

The third method of attack on the ultimate problem is that of stellar distance. Since the opening of the century various new methods of determining the distances of stars have been devised, and in this way it is now possible to speak with some degree of confidence of the scale on which the universe is built. Firstly, as already mentioned, Dr. W. S. Adams, of Mount Wilson, found some years ago that the intensities of certain lines in stellar spectra depend on the absolute luminosity of the stars, and as a result he has been enabled to measure the distances of hundreds of stars by the spectroscope, and thus to add enormously to our knowledge of exact stellar distances. Secondly, Kapteyn, as early as 1901, indicated the possibility of ascertaining the mean parallaxes of groups of stars. Thirdly, Professor H. N. Russell, of Princeton, from his study of ‘ eclipsing-variable ’ stars, was able to ascertain the absolute magnitudes, and thus to compute the parallaxes of nearly a hundred of these. Fourthly, in the large class of variables known as Cepheids, the length of the period of variation has been ascertained to depend on the absolute brightness. The mean parallaxes of the nearer of these variables being known, it is possible to determine the absolute magnitude, and thus the distance of every Cepheid variable in the heavens. Finally, Russell and others have indicated theoretically a method of ascertaining the minimum distance of the bright helium stars of type B. Only stars of very large mass are able to attain to the unusually high temperature of B type stars; hence a B star, however faint it may appear, must have a certain minimum absolute magnitude. All these new methods of determining and estimating distance have contributed to a great extension of our knowledge of the extent and structure of the universe. Further, in recent years it has become possible to ascertain the hypothetical spectral type of the very faint stars. As is well known, the photographic plate is more sensitive than the eye to certain wave-lengths of light; the difference between the photographic and the visual magnitude of a star is therefore due to the colour of the star, and is called the colour-index. Thus it is possible to determine the colour, and hence the approximate spectral type, of very faint stars whose spectra cannot be directly observed.

As a result of these new methods of investigation several researches of the utmost importance have been undertaken within the last few years. Up to the time of his death in 1922, the late Dr. Kapteyn was engaged on an attempt to outline the general galactic system by working from within outwards; that is, by taking the nearer stars and fixing their approximate distances, magnitude by magnitude, by means of the relation between visual magnitude and parallax. Professor Eddington has clearly described Kapteyn's method of procedure thus:

> 'We see the stars projected on the celestial sphere; how can we allot them to their positions in space? How pass from the two-dimensional to the three-dimensional distribution? Some stars are faint because they are distant; others because their luminosity is intrinsically small. The problem which took shape in Kapteyn's mind, with which nearly all his subsequent work was closely bound up, was that of sorting out the stars into different grades of luminosity and different ranges of distance. Direct measurements of distance for individual stars are much too rare and inexact to give any fair idea of the distribution of the stellar population as a whole; but statistically the problem is soluble, and there can be little doubt that the results at which Kapteyn ultimately arrived lead us a great way to the true scheme of distribution. Consider, for example, the stars of the

sixth magnitude ; we can count their number, and by determining their mean parallactic motion—the reflection of the Sun's own motion of 20 km. per sec. among the stars—we can find their mean distance. There remains the somewhat delicate task of determining the law of spread in distance, i. e. what proportion of these individuals will be respectively at $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, 4, &c., times the mean for the whole group ; Kapteyn succeeded in finding this law. Having thus pigeon-holed the sixth magnitude stars, he passed on to the seventh magnitude, eighth, and so on, so far as the observational material extends. Finally, he counted up the contents of each pigeon-hole, and proceeded to formulate the laws of density and luminosity which summarize them.'

Kapteyn calculated two mathematical expressions, the first representing the average parallaxes of stars of a given apparent magnitude, and the second those of stars of a given apparent magnitude and proper motion. The results of his long-continued studies were summarized in several papers published in conjunction with his assistant and successor, Dr. Van Rhijn. Assuming that the Sun was near to the centre of the system, or of that part of the system under discussion, Kapteyn and Van Rhijn calculated the star-density outwards, and were able to draw a section of the galactic system. They concluded that they could assume the reliability of these results up to about 1,500 parsecs. 'In the direction of the pole of the Galaxy this brings us to what many will be inclined to take as practically the limit of the system. . . . In any direction along the plane of the Milky Way, on the contrary, this same limit must be eight times more distant.'

While Kapteyn was engaged on these researches an attack on the problem was made by another method, the method of absolute magnitudes. In 1916 Professor Charlier, of Lund, published his paper on *The Galaxy of the B type Stars,* in which he started from the assumption that the B stars do not differ much in absolute magnitude. Having made this assumption, he was enabled to determine the distances of stars of this type brighter than the fifth magnitude. He found that these stars form a well-defined flattened cluster—

whose plane is very similar to that of the Galaxy—with its centre of gravity in the constellation Carina. The farthest limits of the cluster Charlier found to be at a distance of 750 parsecs—roughly corresponding to about 2,000 light-years. 'We are', said Charlier, 'in a position to get, with the help of the B stars, what might approximately be called a skeleton image of the Milky Way.' In other words, Charlier assumed that the system of B stars was co-extensive with the stellar universe.

In 1913, Hertzsprung, from a study of the Cepheid variable stars in the Lesser Magellanic Cloud, estimated the absolute magnitudes of these stars, and thus their distances and the distance of the cluster. The distance at which he arrived was 10,000 parsecs—corresponding to about 30,000 light-years. In the following year Dr. Harlow Shapley commenced, with the aid of the great 60-inch reflector of the Mount Wilson Observatory, his studies on the colours and magnitudes in stellar clusters. The clusters investigated were of two classes, the compact globular clusters and the open galactic clusters which are undoubtedly condensations of distant Milky Way stars. The results reached by Dr. Shapley during the first two years of his investigation were sufficiently startling. In the galactic star-clouds in the vicinity of the open cluster M 11, he discovered faint blue stars obviously of the type B.

> 'The cluster stars', wrote he in April 1917, 'are probably giants in luminosity, and accordingly the distance of the group must be of the order of 15,000 light-years. The wide dispersion in magnitude of both blue and red stars indicates a similarly great distance for the neighbouring galactic clouds. It suggests that the extent of the stellar clouds in the line of sight is relatively very great—in fact, the depth may be as great as, or greater than, the distance to the nearer boundary.'

No less important were Shapley's preliminary conclusions regarding the brighter globular clusters. The first of these exhaustively studied was the great cluster in Hercules (M 13). In his paper on this cluster, dated August 1915, Shapley estimated its distance from ‘considerations of its variable-stars, its fragmentary luminosity-curves, and the average brightness of certain colour-groups’, as 100,000 light-years,

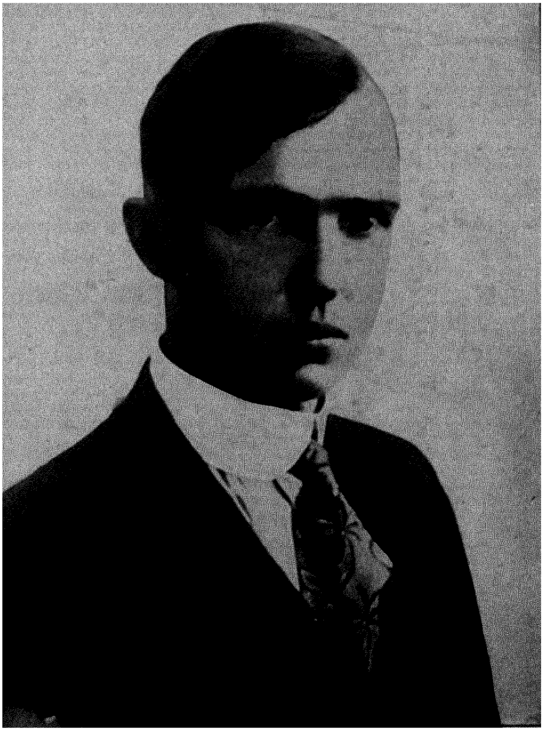

FIG. 22. HARLOW SHAPLEY

a distance much greater than had ever been imagined for the cluster. A revised measure by Dr. Shapley gave a distance of 36,000 light-years. Shapley's subsequent researches have been given to the world in the series of *Contributions from the Mount Wilson Observatory*, and have been admirably summarized in his classic pamphlet on *Star-Clusters and the*

Structure of the Universe. By means of the various methods of determining absolute magnitude and distance, Shapley has fixed the positions in space of eighty-six globular clusters. He finds these to be 'cosmic units', sub-systems dependent on the greater galactic system, at enormous distances from the Earth. The nearest, Omega Centauri, is 20,000 light-years away, and the most distant—known as N.G.C. 7,006—is so distant that light requires 220,000 years to travel from the cluster to the Earth.

In 1924 and 1925 Dr. Shapley succeeded in getting reliable measures of the distances of the Magellanic Clouds, which may be grouped with the clusters as subordinate cosmic units. He found the distance of the Small Cloud to be 105,000 light-years, and of the Large Cloud 112,000 light-years. As the result of an independent investigation, Dr. Lundmark, of Upsala, in 1924, fixed 100,000 light-years as the distance of the latter object, a result in close agreement with that of Dr. Shapley. In November 1923 Dr. Shapley drew attention to a faint, nebulous-looking object closely resembling the Magellanic Clouds—N.G.C. 6,822. For this distant system he fixed an approximate distance of a million light-years. Shapley found the clusters to be arranged in a certain symmetrical way, which indicates that they are dependants of the sidereal system, as Perrine suggested in 1917, and that the Sun is not near the centre of the universe, as was generally believed. Shapley's researches indicate that the centre of gravity of the sidereal system is in the constellation Sagittarius, amid the dense star-clouds of that region, 60,000 light-years from the Sun. He considers the stars in the vicinity of the Sun—including Charlier's galaxy of B-type stars, which that astronomer thought to be co-extensive with the universe—to form a local cluster—a striking confirmation of the theory put forward years ago by B. A. Gould. The stellar system studied by Charlier and by Kapteyn from different standpoints would appear to be simply the local cluster. It is the centre of gravity of this local

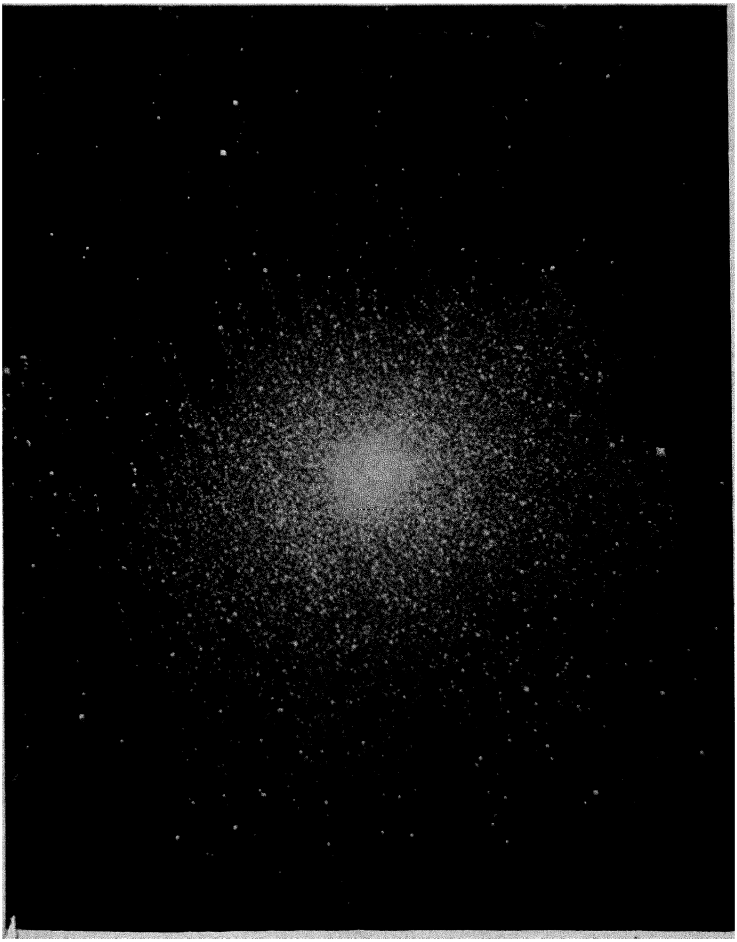

FIG. 23. THE GREAT CLUSTER IN HERCULES

Photograph by Mount Wilson Observatory cluster which happens to be near to the Sun's present place in space.

The stellar system itself is seen to be much vaster than has been hitherto believed. 'The study of the distances and structure of globular clusters has shown that in volume the galactic system is more than a hundred thousand times as large as we formerly believed it to be'—at least 300,000 light-years in diameter.

> 'The phenomenon of the Milky Way is largely an optical one. Although the existence of local and occasionally very extensive condensations of Milky Way stars is not denied, the conception of a narrow encircling ring is abandoned. The Milky Way girdle is chiefly a matter of star-depth, and its long-recognized weakness between longitudes 90° and 180° is now taken to be a reflection of the eccentric portion of the Sun.'

The system is vastly more extended in the galactic plane than in the direction of the poles. 'A thin central stratum of the galactic segment contains every star that has been seen or has been photographed for our catalogues. This stratum of stars apparently deviates less than 2,000 light-years from the galactic plane.' In other words, Shapley's researches show the sidereal system to be a flattened disc, about 4,000 light-years in thickness and 300,000 in diameter, containing, according to Seares and Van Rhijn, about 1,000 million stars down to the twentieth visual magnitude.

In the course of his work Shapley was impressed by the absence of globular clusters from the mid-galactic regions, a fact which in his view cannot be explained solely by the presence of dark obscuring matter. Slipher's measurements of the radial velocities of the globular clusters indicate a preponderating motion of approach. This suggested to Shapley the hypothesis that the clusters are being drawn gradually into the greater galactic system. Support is accorded to this view by the fact that the globular clusters nearest to the galactic plane are the least condensed; their component stars would appear to be dispersing under the overwhelming gravitational force of the stellar system. If this theory be

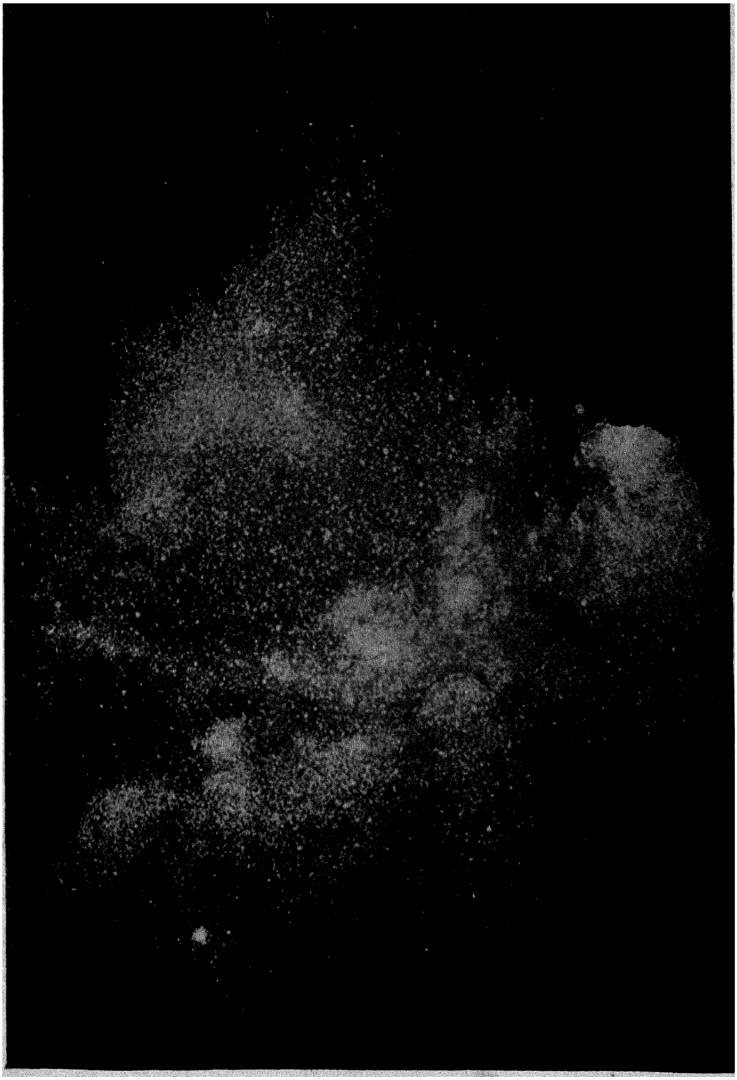

FIG. 24. THE STAR-CLOUDS OF THE GALAXY IN SAGITTARIUS AND SERPENS

Photograph by Dr. Max Wolf

correct, the numerous open clusters which are intermingled with the galactic star-fields are simply globular clusters which have been absorbed into the main system. When a cluster is thus absorbed, Shapley remarks, two results follow.

> ‘ Faint stars in globular clusters are of small mass and of more than average velocity, and in their orbital motions frequently attain great distances from the centre. When the globular cluster approaches a disturbing body as massive as the general galactic system, such stars are of course most readily lost and intermingled with galactic stars. On the other hand, the massive cluster stars which are mostly of high luminosity, having low peculiar velocities, and, maintaining, in their sub-system a high degree of stability, retain their organization in a disrupting field.’

This suggests that the local cluster of which the Sun is a member may have at one time formed part of a globular cluster which has been absorbed by the greater system. Indeed, it is not improbable that the galactic system, as we know it, had its origin in the intermingling of two clusters, and grew to its present huge dimensions by absorbing other clusters, a process which, if this hypothesis is correct, is still proceeding. The time required for this vast cosmic progress is almost unthinkable ; an estimate of thousands of millions of years probably errs on the side of caution.

The distances assigned by Dr. Shapley to the globular clusters have been disputed by several astronomers. Thus, Kapteyn and Van Rhijn challenged Shapley's assumption that the short-period variable stars in star-clusters are giant stars analogous to the Cepheid variables in the stellar system. Kapteyn and Van Rhijn emphasized the possibility that the variables in clusters are really dwarfs, and that the distances of the clusters may be only one-seventh the distances computed by the American astronomer. But several independent investigations militate strongly in favour of Dr. Shapley's scale of distances. Thus, in 1922, Dr. Lindblad, a Swedish astronomer, devised a new method of finding the absolute magnitudes of stars from their spectra ; in this way he found the brightest stars in the Hercules cluster and in M 3 to be giants. This exactly agrees with Shapley's results. Further, Mr. Strómberg, in a paper read before the Washington Academy in April 1925, found from a refined discussion of the motions of the stars in the galactic system that the motions of the stars in our neighbourhood are determined not only by the gravitational field due to a local system of stars, but also by another field connected with the system of globular clusters and spiral nebulae, a conclusion which agrees with Shapley's conclusions as to the status of the former systems. Further, Dr. Lundmark points out that the Sun is evidently moving in a direction at right angles to the Sagittarius region. 'This may indicate a rotation of our local cluster in the plane of the Milky Way around the Sagittarius region.' On the whole, it may be said that the balance of evidence is strongly in favour of the substantial accuracy of Dr. Shapley's scale for the Universe, and his conclusions gain weight daily.

The status of the spiral nebulae is one of the most baffling problems of astronomy. Are these enigmatical objects true nebulae, or are they island universes, assemblages of stars co-equal to our stellar system sunk in space at almost inconceivable distances ? The ' island universe ' theory came into favour towards the close of the first decade of the century. In 1912, Dr. V. M. Slipher commenced at the Lowell Observatory his measures of the radial velocities of these objects. Grave doubts had already been thrown by spectroscopic evidence on the supposed gaseous nature of these nebulae. Slipher's investigations indicated that ' the average velocity of the spirals is about twenty-five times the average stellar velocity ', a fact which differentiates them sharply from the other nebulae. Unlike the true nebulae, they avoid the galactic plane, and are most numerous near the galactic poles. These facts seemed to emphasize the non-nebular, extra-galactic nature of the spirals.

On the other hand, the other facts did not fit in with the island universe theory. Their avoidance of the galactic plane seemed to indicate some definite relation to the stellar system, which they would not have if they were independent co-equal objects. More serious still were the objections based on Dr. Van Maanen's supposed measures of internal motions, which indicated comparative proximity, and by Dr. Shapley's argument that the novae seen in spirals would have on the island universe theory a luminosity greater than that of any star in our stellar system. Nevertheless, the partial resolution into stars of M 31 (the Andromeda nebula) and M 33 by Hubble at Mount Wilson would seem to be decisive evidence that these at least are external systems, and Dr. Shapley, in a communication to me, has stated that the Mount Wilson work is fairly conclusive. 'I am accepting the conclusion that the brighter spiral nebulae are external stellar systems.' But he adds: 'I should emphasize that the dimensions of Messier 33 are similar to those of the Magellanic Clouds, and therefore not one-tenth as big as the dimensions of our own Galaxy. The same is roughly true of the Andromeda nebula.'

Dr. Hubble has fixed the distances of these two spirals at a million light-years, which agrees in the case of M 31 with the result reached in 1919 by Messrs. Lindemann, who by means of the photo-electric photometer reached an identical figure, and with a recent independent measure by Dr. Öpik, of Tartu (Dorpat). These are the nearest of the spirals. Dr. Lundmark has recently computed the distance of the globular nebula N.G.C. 4481 from the faint stars visible in Dr. Hubble's photograph of that object. His value is eight million light-years. The dimensions are only 6,000 light-years, so that the stars in this system are closely packed and the dimensions only a tenth of the dimensions of the Andromeda nebula. Anyhow, the object is too large to be comparable with a globular cluster, and it is more likely that we have in the globular nebulae the forerunners of the Milky Way systems. For N.G.C. 4594 Dr. Lundmark finds a distance of 58 million light-years.

Admitting that these two latter results are very hypothetical, we are forced to admit that we have evidence of objects at distances of one million light-years, i. e. six million billion miles. Naturally, we are led to ask if there is any end to

> 'this interminable wilderness of worlds,
> At whose immensity even soaring fancy staggers'.[1]

Professor Einstein concludes from his general theory of relativity that the Universe is 'finite yet unbounded'. It is deduced from the theory that space is curved—spherical or elliptical; it is of finite volume and without bounds. The difficulty of this conception, as Eddington remarks, is 'that we try to realize this spherical world by imagining how it would appear to us and to our measurements. There has been nothing in our experience to compare it with, and it sounds fantastic. But if we could get rid of the personal point of view and regard the sphericity of the world as a statement of the type of the order of events outside us, we should think that it was a simple and natural order which is as likely as any other to occur in the world.' On the assumption that the Universe is finite, its size has been computed by Einstein and also by his able co-worker in this field, Dr. De Sitter, of Leyden. Both agree that a ray of light would require 1,000 million years to go 'round the world.' If it were possible for a ray of light to perform such a journey, some remarkable consequences would ensue. After 1,000 million years the rays of sunlight and starlight would return to their starting-points. In theory, therefore, the sky should be covered with 'stellar ghosts' in the very positions occupied by the Sun and stars 1,000, 2,000, or 3,000 millions of years ago. It has even been suggested that some of the spiral nebulae may be phantoms of our own stellar system. The likelihood that this would happen, however, is exceedingly slight. It is almost incredible that the rays of light after so long a journey would converge at the very point at which they diverged. Theoretically it is possible, but the probability is that the light would either be absorbed or scattered, or else deflected by the various gravitational fields throughout space.

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[1] Shelley, *Queen Mab*.

Even if Einstein's view be correct, if the space we know in association with matter is ' finite yet unbounded ', we have no reason to believe that it exhausts the All. It may be but a transient manifestation ; there may be more than poetry in Shelley's fine words :

> ' What is heaven ? A globe of dew, . . .
> A frail and fading sphere
> With ten millions gathered there,
> To tremble, gleam, and disappear.'

X

COSMOGONY

In pre-scientific days, cosmology and cosmogony were inextricably intermingled ; the old mythologies were indeed curious compounds of theology and theogony, of cosmology and cosmogony. Even the Greek thinkers of the Ionian school were not deterred by their slender knowledge of the world from putting forward hypotheses of the origin of the world-system. Thus Thales, Anaximander, Anaximenes, and Heraclitus each described the development of the Cosmos out of the primal substances in which they believed ; Leucippus, Democritus, and later Lucretius, each found in the primordial atom the explanation of the origin of things.

With the rise of a school of exact science, cosmology and cosmogony parted company. The great Greek astronomers, Eudoxus, Hipparchus, and their successors, did not concern themselves with cosmogony. Even in the philosophic schools, the fruitful ideas of evolution and development were practically forgotten. The post-Socratic thinkers in Greece and the Christian apologists and philosophers did not trouble about the origin of things.

After the conversion of the old Empire to Christianity—or rather, after the acceptance of a diluted form of Christianity as the official religion of the Empire—the Hebrew cosmogony contained in the first chapter of Genesis was accepted as the final word on the method of creation; and so the vague notions of the enlightened Greek philosophers gave way to the theory of six literal days of creation and a 'carpenter God'. Even after the universal acceptance of the Copernican system, this theory of creation out of nothing in six literal days held its ground. The reason of this is not far to seek. The great men of science—Copernicus, Tycho Brahe, Kepler, Galileo, Huyghens, Newton—were fully occupied with the empirical study of nature. They were too much absorbed in the task of understanding things as they are to care particularly as to how they came to be.

Curiously enough, it was a theologian, Ralph Cudworth, who, in 1678, in his *Intellectual System of the Universe,* argued against the prevalent mechanical theory of a literal and finished creation, and in favour of the origin of the world-system as a result of development in accordance with an immanent principle. In the eighteenth century, the famous Scottish astronomer, James Ferguson, threw out a remarkable hint as to the building-up of the Solar System as a result of gravitational action.

> 'In the beginning', wrote Ferguson, 'God brought all the particles of matter into being in those parts of open space where the Sun and planets were to be formed, and endowed each particle with an attractive power, by which these neighbouring and at first detached particles would in time come together in their respective parts of space, and would form the different bodies of the Solar System.'

The famous nebular hypothesis seems to have originated independently in the minds of three men—Kant, Laplace, and Herschel. There is nothing to show that Laplace and Herschel were acquainted with Kant's speculation, or with the cosmogonic views of each other. The Kantian hypothesis was given to the world in his 'Universal Natural History and Theory of the Heavens, or an Essay on the Constitution and Mechanical Origin of the whole Universe, treated according to Newton's Principles'. Owing to the bankruptcy of the publisher the book never appeared, and Kant's speculations remained in obscurity, save for a summary which he appended to one of his metaphysical works. Kant admitted his indebtedness in his cosmogonic speculations to the atomist philosophers, Democritus and Lucretius. He conceived of a primeval chaos of atoms, but, unlike his predecessors, he refused to admit that the concourse of these atoms was without meaning or purpose. By means of gravitation and molecular repulsion, these atoms were condensed in the course of ages into the bodies of the Solar System—the Sun consisting simply of the aggregation of atoms at the central point of the nebula, and the planets of subordinate centres of condensation, while the collision of the particles drawn towards the central mass was supposed to have produced the orbital velocities of the various planets.

In 1796, over forty years after Kant's essay was written, Laplace enunciated the theory generally known as 'the nebular hypothesis'. The hypothesis was advanced in the closing chapter of his popular book, *Système du Monde*, 'with that distrust which everything ought to inspire that is not the result of observation or calculation'. Laplace began by a consideration of the Solar System as it is at the present time. 'We are astonished', he said, 'to see all the planets move round the Sun from west to east, and nearly in the same plane, all the satellites moving round their respective planets in the same direction and nearly in the same plane with the planets.' The Sun and planets, too, he pointed out, rotate on their axis in the same direction, from west to east. 'A phenomenon so extraordinary is not the effect of chance; it indicates a universal cause, which has determined all these motions.'

Thus Laplace reached his conclusion by reasoning backward from the remarkable congruities in the planetary system. The cause of these, he concluded, 'must have been a fluid of immense extent'. To have given in the same direction a motion nearly circular round the Sun, the fluid must have been a kind of solar atmosphere, which at one time extended far beyond the limits of the present Solar System. As the 'atmosphere' or nebula contracted, the planets were formed by 'the condensation of zones', while the satellites were formed in a similar way. The five remarkable congruities in the planetary motion, and in the inclination of the planetary orbits, which Laplace pointed out, naturally result from this hypothesis, 'to which the rings of Saturn add an additional degree of probability'.

It is of interest to compare the theory of Laplace with that of Kant. Kant's was a broad generalization, while Laplace went into details, and naturally a broad generalization has less chance of proving faulty. Similarly with the nebular theory of Herschel, who, five years before Laplace put forward his theory, had been led to the vague idea of celestial evolution. On 10 February 1791, Herschel communicated to the Royal Society an important paper, 'On Nebulous Stars, properly so-called'. In the earlier stages of his investigations into the construction of the heavens, Herschel had believed that the nebulae were actually clusters of stars, at distances so great that the component stars were invisible to the unaided eye. He assumed the truth of this surmise until he began a close study of the remarkable objects known as nebulous stars. He commenced his paper with the words:

> 'In one of my late examinations of a space in the heavens, which I had not reviewed before, I discovered a star of about the eighth magnitude, surrounded with a faintly luminous atmosphere of a considerable extent. The phenomenon was so striking that I could not help reflecting upon the circumstances that attended it, which appeared to me to be of a very instructive nature and such as may lead to inferences which will throw a considerable light on some points relating to the construction of the heavens.'

After a consideration of the nebulous star in Taurus, to which he directed attention, he declared that 'the nebulosity about the star is not of a starry nature'.

Herschel's expression of opinion was no mere surmise. He clearly proved the impossibility of these nebulous clouds consisting of stars. If, he pointed out, the nebulosity consisted of stars that are very remote, which appear nebulous on account of the small angles their mutual distances subtend, ‘ then what must be the enormous size of the central point which outshines all the rest in so superlative a degree as to admit of no comparison ? ’ If, however, the star is of average size, the points composing the nebulosity must be almost infinitesimal. ‘ We therefore either have a central body which is not a star, or have a star which is involved in a shining fluid of a nature totally unknown to us. I can adopt no other sentiment than the latter.’

From this Herschel concluded that it had been decided with undue haste that the ordinary nebulae were merely star-clusters at enormous distance. We may thus, he said, ‘ with great facility explain that very extensive telescopic nebulosity ’ in the constellation Orion. Thus, he concluded, that ‘ shining fluid ’ can exist without a star. The independence of nebulous stars of stars proper is thus ‘ fully proved ’. Then, with characteristic caution, he added, ‘ If, therefore, this matter is self-luminous, it seems more fit to produce a star by its condensation than to depend on the star for its existence.’ This was in 1791, five years before the publication of the *Système du Monde*. So far as we know, Laplace and Herschel worked on quite independent lines.

In 1811, in a paper on the ‘ Construction of the Heavens ’, Herschel once more dealt with the development of nebulae, in a more exhaustive manner. Here he gave a complete list of nebulae, which he had discovered and studied, ‘ assorting them into as many classes as will be required to produce the most gradual affinity between the individuals contained in any one class with those contained in that which precedes and that which follows it ’. Those contained in one class and those in the next class in order, he declared, have not so much difference between them, in his own suggestive remark, ‘ as there would be in an annual description of the human figure, were it given from the birth of a child till he comes to be

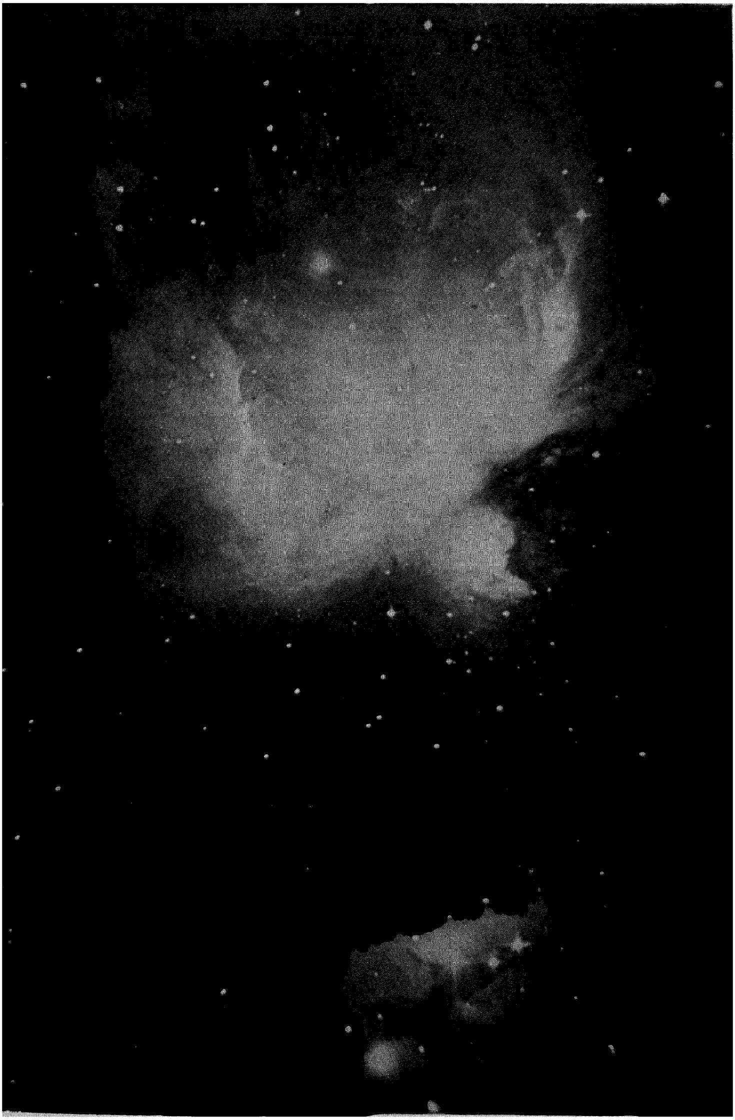

FIG. 25. THE ORION NEBULA

Photograph by Dr. Max Wolf

a man in his prime'. Beginning with extensive diffused nebulosities, he proceeded by way of milky nebulae, 'milky nebulae with condensations', double nebulae, irregular nebulae, nebulae of 'an irregular round figure', round nebulae, nebulae 'a little brighter in the middle', 'gradually brighter in the middle', 'much brighter in the middle', nebulae with a nucleus in the middle, nebulae showing the progression of condensation, planetary nebulae, stellar nebulae, to nebulae nearly approaching to the appearance of stars. He declared it highly probable that 'every succeeding state of the nebulous matter is the result of the action of gravitation upon it while in a foregoing one, and by such steps the successive condensation of it has been brought up to the planetary condition. From this the transit to the stellar form, it has been shown, requires but a very small additional compression of the nebulous matter.' He also rebutted the argument that the stars and presumably also the Sun could not have been formed from nebulous matter.

At first the speculations both of Laplace and Herschel were received with indifference. Laplace had promulgated his views simply as the appendix to a popular book. Herschel, who wrote obscurely and in an imperfect English style—for he was, after all, a typical German—did not give his theory to the world in book form; his views on nebulae were scattered through the volumes of the *Philosophical Transactions* of the Royal Society. On the whole, however, the nebular hypothesis was scarcely treated with the attention it deserved. About the middle of the nineteenth century it was indeed believed to have been discredited. The theory was based on the non-stellar nature of nebulae—the existence of the 'shining fluid'. About 1845, the great telescope of Lord Rosse and large refractor at Pulkova resolved a number of so-called nebulae into stars, and so the belief gained ground that all nebulae would be resolved in course of time. And so the American astronomer Olmsted expressed the prevailing opinion of astronomers when he said that the resolution of these nebulae was the 'signal for the renunciation of Herschel's nebular hypothesis'. Only a few bold thinkers, such as Herbert Spencer, continued to believe in gaseous matter in space.

In 1859 came Kirchhoff's preliminary investigations with the aid of the spectroscope. The seemingly impossible had been accomplished; it was possible to ascertain the elements of which the celestial bodies were composed. Rarely has one observer been placed in so thrilling a position as was Huggins when, on 29 August 1864, he turned the spectroscope on a planetary nebula in the constellation Draco.

> 'The reader', said Huggins, in a paper published long afterwards, 'may now be able to picture to himself to some extent the feeling of excited suspense, mingled with a degree of awe, with which, after a few moments of hesitation, I put my eye to the spectroscope. Was I not about to look into a secret place of creation? I looked into the spectroscope. No spectrum such as I expected! A single bright line only. . . . The riddle of the nebulae was solved. The answer which had come to us in the light itself read: Not an aggregation of stars, but a luminous gas. . . . There remained no room for doubt that the nebulae, which our telescopes reveal to us, are the early stages of long processions of cosmical events, which correspond broadly to those required by the nebular hypothesis in one or other of its forms.'

The formulation of the doctrine of the conservation of energy, too, lent strong support to the nebular hypothesis, for Helmholtz deduced from the principle that the heat of the Sun was generated by the motion of contraction of the solar globe. In fact, calculating from the rate of contraction, Helmholtz assigned to the Sun a past life of twenty-two million years, and a future of seventeen million. Evidently, if the central body of the Solar System is still contracting, it must have been much more widely extended in the past; reasoning backwards, we come to the time when it was a nebula.

Various obstacles to the unqualified acceptance of the Laplacian theory were obvious even before the death of its author. On the original theory, no body in the Solar System could revolve in a retrograde direction; yet, during Laplace's lifetime, Herschel had discovered the retrograde motions of the Uranian satellites. A hundred years later other three instances of retrograde satellites were discovered—the ninth satellite of Saturn by W. H. Pickering in 1898, and the two outermost moons of Jupiter at Greenwich and the Lick Observatory in 1908 and 1914. In 1861 it was shown by Babinet, a French mathematician, that the axial motion of the supposed solar nebula extending to the orbit of Neptune would have been so slow that 27,000 centuries would have been required for a single revolution. Under such conditions, the centrifugal force could never have counterbalanced the attractive force, and as a consequence there could have been no detachment of rings or separation of planetary bodies from the parent mass. Proctor, too, pointed out in 1874 that 'Laplace's great nebulous contracting mass is a very unsatisfactory conception to begin with. Laplace's theory does not in any way correspond with processes taking place within the Solar System. It gives no account of the immense number of meteor flights and comets still existing within the solar domain.' And the late Sir G. H. Darwin remarked that a ring of matter distributed uniformly would in all likelihood collapse on the mass from which it was detached. According to the Laplacian theory, too, satellites must revolve round their primaries more slowly than the latter rotate on their axes. The central body in contracting rotates more and more swiftly. The inner of the two satellites of Mars, discovered in 1877, revolves three times for one rotation of its primary. Similar remarks apply to the inner ring of Saturn, the meteoric components of which revolve in about half the time required by the planet to rotate on its axis.

Any one of these difficulties might perhaps in itself be overcome, but their cumulative effect is fatal—not to the nebular hypothesis as such, but to Laplace's particular form of the theory. Professor Moulton, of Chicago, has shown conclusively that matter detached from a rotating spheroid of gas would be 'shed continually'; in other words, no separate rings could be formed such as Laplace imagined. Moulton has also pointed out that the distribution of the planets according to mass hints at a nebula not homogeneous, but heterogeneous. The question of the exact degree of heterogeneity has been answered in various ways. Proctor, who made a special study of cometary and meteoritic phenomena, pointed out in 1870 the important part played by meteoric aggregation in the formation of the planets. Twenty years later this idea was worked up into an elaborate theory by Lockyer, in his book entitled *The Meteoritic Hypothesis.* The theory has been called 'perhaps the most comprehensive cosmogonic guess that has ever been attempted'. Lockyer, basing his ideas largely on Schiaparelli's discussions on 'the local concentration of celestial matter', concluded that 'all self-luminous bodies in the celestial space are composed either of swarms of meteorites or of masses of meteoritic vapour produced by heat. The heat is brought about by the condensation of meteor swarms due to gravity, the vapour being finally condensed into a solid globe.'

The defect of the theory lay in its precision; Lockyer aspired to describe the evolution of the cosmos with too much detail. He described nebulae, for instance, as vast aggregations of meteorites in rapid motion, constantly coming into collision; but this was disputed by Newcomb, who pointed out that 'every time that two meteors came into collision they would lose velocity, and therefore if the mass was sufficient to hold them from flying through space, would rapidly fall towards a common centre'. The cool reception of the hypothesis, however, was chiefly due to the fact that on one important point it was brought to the test of instrumental investigation and refuted. Lockyer contended that the chief line in the spectrum of the Orion nebula coincided in position with the remnant of the magnesium fluting, which is conspicuous in cometary and meteoric spectra. Had this been actually verified much could have been said for the meteoric constitution of nebulae. Huggins in London, and

Keeler at the Lick Observatory, however, showed that the two lines did not coincide.

While Lockyer was working at his cosmogony, G. H. Darwin was engaged in the development of his theory of tidal friction. His first paper on the subject was contributed to the Royal Society in 1879, and in 1898 he summed up his investigations in his book on *The Tides*. He found that the tides act upon the Earth as a brake on a machine; they tend to retard the terrestrial rotation and thus to lengthen the day. Consequently, the orbit of the Moon is becoming enlarged, and as a result the period of its revolution is gradually growing longer. Calculating backwards, Darwin was enabled to point to a time in the remote past when the Earth was probably rotating on its axis in a very short period, between three and five hours in length. The Moon, which was much closer to the Earth than now, revolved round its primary in the same period that the latter required to rotate on its axis. The two globes, then gaseous, must have been revolving almost in actual contact. At a still earlier period, Darwin found, the two bodies were probably one. The Moon originally formed a part of the Earth, which, owing to its rapid rotation and probably also owing to the interference of the solar tide, split into two portions, of which the smaller now forms the Moon; or, as is more probable, the matter now forming the Moon was separated from the Earth in the form of small masses. As a result of the tidal action of the Earth on the Moon's molten surface, the rotation of our satellite was retarded, and the Moon was gradually forced further and further from the Earth until it reached its present position.

Following up Darwin's work, Professor W. H. Pickering considers that 'it will be of interest to determine if possible from what part of the Earth the Moon originated'. 'When the Earth-Moon planet condensed from the original nebula, its denser materials collected at the lower levels, while the lighter ones were distributed with considerable uniformity over its surface. At the present time we find the lighter materials missing from one hemisphere. We find a large mass of material now up in the sky, which, it is generally believed by astronomers, formerly formed part of the Earth, and the density of this material we find to be not far from that of the missing continents. From this we conclude that this mass of material formerly covered that part of the Earth where the continents are lacking, and which is now occupied by the Pacific Ocean.'

About the beginning of the present century, two American physicists, Messrs. Chamberlin and Moulton, put forward the 'planetesimal hypothesis' of the development of the Solar System from a spiral nebula. This nebula, unlike Laplace's homogeneous mass of gas, would have, on their theory, almost any degree of heterogeneity. It is claimed on behalf of this hypothesis that it explains away all the old stumbling-blocks to Laplace's theory. According to the 'planetesimal theory', the planes of the planetary orbits 'will nearly, though not exactly, coincide', the orbits of the smaller planets, like those of the asteroids, will show a greater inclination to the general plane than those of the larger; the orbits of the larger planets will be more nearly circular than those of the smaller; retrograde rotations may be expected in the outer planets, and retrograde revolutions in satellites very remote from their primaries. The theory was very heartily accepted by the late Mr. Gore shortly before his death. He claimed that 'the spiral theory is far superior to Laplace's nebular hypothesis. Spiral nebulae were, of course, unknown to Laplace, and, had he known of their existence, we should probably never have heard of ring formation'.

Chamberlin and Moulton sought to explain not only the development of planetary systems from spiral nebulae, but the origin of these nebulae themselves. A spiral nebula—on their hypothesis—may be formed by the approach or collision of two solid masses. The near approach of a large mass to another of larger size would result in the production on the latter body of a gigantic tide on the side nearest to the approaching mass. Another great tide will also be formed on the opposite side. As a result of these tides, there will be explosions, and the ejected nebulous material will necessarily, according to Moulton, assume the spiral form. Dr. See, the well-known American astronomer, advanced about 1911, as a kind of corollary to this cosmogony, what is known as the 'capture theory'. No such process, he concluded, 'as the detachment of Laplacian rings or zones of vapour in the Solar System ever took place; but, on the contrary, the planets were added on to the Sun and the satellites on to their several planets, the nuclei in every case having originated in the distance and subsequently approached the centres about which they now revolve'.

The spectroscopic study of the stars had led astronomers by the close of the nineteenth century to fairly definite ideas of the course of stellar evolution. In 1874, Vogel, when formulating a more elaborate classification of stellar spectra than that of Secchi, proceeded from the standpoint 'that the phase of development of the particular body is in general mirrored in its spectrum'. In 1895 Vogel revised his classification to make room for the newly-recognized type of helium stars. These were believed by Vogel and Huggins to be the connecting-link between stars and nebulae, the evolutionary order proceeding by way of gaseous nebulae and helium stars through Sirian and solar stars to the red stars which Vogel thought to be 'effete suns hastening rapidly down the road to final extinction'. Vogel's tentative scheme formed the theoretical basis of the elaborate Draper classification carried through by E. C. Pickering and his assistants at Harvard. In this classification, based on the observed spectra, which seem to shade one into another, the different types are designated by letters of the alphabet—OBAFGKMN. This is called the 'Harvard sequence'.

That the Harvard sequence represented the order of stellar evolution was the all but unanimous opinion of astronomers about twelve years ago. Strong support, too, seemed to be lent to it by the discovery, made independently by Kapteyn and Campbell in 1910, that the radial velocities of stars of the later types were greater than those of the earlier types. These two astronomers demonstrated that the average linear velocity increases from one type to another through the Harvard sub-divisions BAFGKM. On this discovery Eddington remarked in 1911:

> 'This relation of spectral type and velocity is one of the most startling results of modern astronomy. For the last forty years, astrophysicists have been studying the spectra and forming their systems by which they arrange the stars in order of evolution. However plausible may be their arguments, one would have said that their hypotheses must be for ever outside the possibility of confirmation. Yet if this result is right we have a totally different criterion by which the stars are arranged in the same order.'

It was just at this time, however, that evidence began to be collected which cast doubt upon the generally-accepted order of evolution. Hertzsprung, of Potsdam, had drawn attention in 1905 to the existence of two well-defined classes of stars, which he named 'giants' and 'dwarfs'; five years later the same astronomer remarked that there is a certain limit of brightness for each spectral class, below which stars of this class are very rare, if they occur at all. At the same time, Dr. H. N. Russell, of Princeton, commenced the series of investigations which he described fully in lectures before the Royal Astronomical Society and the American Association for the Advancement of Science in 1913. Russell, from statistical results and parallax investigations, recognized Hertzsprung's two classes of giants and dwarfs among the red stars.

> 'There are', he said, in December 1913, 'two great classes of stars—the one of great brightness, averaging perhaps a hundred times as bright as the Sun, and varying little in brightness from one class of spectrum to another; the other of smaller brightness, which falls off very rapidly with increasing redness. . . . The two groups, on account of the considerable internal differences in each, are only distinctly separated among the stars of class K or redder. In class F they are partially, and in class A thoroughly intermingled, while the stars of class B may be regarded equally well as belonging to either series.'

Doubt was cast in Russell's mind on the generally accepted order of evolution by Shapley's work on eclipsing binaries, which indicated a greater density for stars of Secchi's first type than for those of the second.

> ' The order of increasing density ', said Russell, ' is the order of advancing evolution. . . . The giant stars then represent successive changes in the heating up of a body and must be more primitive the redder they are ; the dwarf stars represent successive stages in its later cooling, and the redder of these are the furthest advanced. We have no longer two separate series to deal with, but a single one, beginning and ending with class M, and with class B in the middle—all the intervening classes being represented in inverse order in each half of the sequence.'

Russell's theory showed that the Lane-Ritter law applies to the giant stars on the up-grade ; as they grow hotter, they contract. This goes on until a certain critical stage is reached, when the star becomes too derse to obey the laws of a perfect gas ; then the temperature begins to fall and the star's brightness decreases rapidly. Two well-known facts fit into the theory. (1) The relative scarcity of giant stars of class B (Vogel's type O) is accounted for by the fact that only stars of very large mass are able to attain the unusually high temperature of B-type stars. ' Only these stars would pass through the whole series of the spectral classes from M to B and back again in the course of their evolution. Less massive bodies would not reach a higher temperature than that corresponding to a spectrum of class A, those still less massive would not get above class F, and so on.' (2) ' It is now easy to understand why there is no evidence of the existence of luminous stars of mass less than one-tenth that of the Sun. Smaller bodies presumably do not rise, even at a maximum, to a temperature high enough to enable them to shine perceptibly from the stellar standpoint, and we do not see them.'

It was evident from the time of the promulgation of Russell's theory that the position of the nebulae had become very problematical. ' Nebulae and red stars ', as Campbell puts it, ' do not co-exist.' The recent work of Hubble has shown that in many cases the great extended nebulae owe their radiation to the presence of bright stars, and that they are, indeed, but particular cases of dark nebulae which seem to be the normal type of primeval matter. The researches of Wolf, Barnard, and others have shown these dark nebulae to be very prevalent, especially in the galactic plane. Accordingly, Russell believes that it is from these great masses of dark nebular matter that the giant red stars are born.

Russell's theory was strongly confirmed by the work of Dr. Adams at Mount Wilson in 1916. Adams proved the existence of two groups of M stars. Later came the measurement of the diameter and actual size of Betelgeux, in 1920. Strong theoretical support was lent to Dr. Russell's hypothesis by the investigations of Professor Eddington in 1916 and 1917. In these investigations Eddington dealt with the giant stars in which alone the law of a perfect gas was assumed to hold. In his attack on the problem of the interior of the giant stars he considered the balance between gravitation which tends to induce contraction, and the forces resisting it. He showed that the resistance arose not only from the elasticity of the internal parts, but also from radiation pressure. This indicated that there is a limiting value for the masses of the stars. In the star with mass beyond that value, the radiation pressure, tending to disrupt the star, would become dangerously large, and the star would be unlikely to survive. This theoretical work strongly supported Russell's hypothesis, according to which the masses of the stars must lie within narrow limits.

Applying the theory to Cepheid variables, Eddington found that the rate of contraction of these stars was much too slow to provide the heat indicated. Consequently, the contraction theory of the source of stellar heat is shown to be quite inadequate, and the stars are very much older than they were formerly believed to be. What the source is, is not known with any degree of certainty. ' A star is drawing on some vast reservoir of energy by means unknown to us. The reservoir can scarcely be other than the sub-atomic energy which it is known exists abundantly in all matter.'

Eddington showed further that in the case of the giants the star's luminosity is a function of the mass, which agreed well with Russell's theory of evolution and with the observed fact that the giant stars are of fairly equal absolute magnitude throughout the various spectral types. Eddington's most recent work, however, throws grave doubt on the theoretical validity of Russell's theory. In a paper on the masses and luminosities of the stars, completed early in 1924, Eddington showed that the assumption that the dwarf stars did not obey the perfect gas law was in all likelihood unsound. The fact of ionization—the loss of electrons by atoms at high temperature—had been neglected in previous investigations, and it had been assumed that a star, when its density reaches about a tenth that of water, ceased to behave as a perfect gas. ' This ', says Eddington, ' is based on a false analogy between the stellar ions and ordinary atoms. There is no physical reason to expect that dwarf stars like our Sun will behave otherwise than as a perfect gas.'

Quite evidently, if the perfect gas laws are obeyed by the dwarfs, Russell's original theory breaks down. The cardinal point of that theory is, as Eddington remarks, ' that the transition from the giant to the dwarf stage was due to the failure of the perfect gas laws when the density became too great '. But Eddington has suggested an alternative theory, according to which the stars ' take a course of evolution similar to that indicated by the giant and dwarf theory, though for different reasons '. The stars, according to this view, lose mass as they grow older ; they burn themselves away. But evolution is due to loss of mass, not, as on Russell's original theory, to a change from compressibility to incompressibility.

After Eddington's more recent conclusions were announced, Russell proceeded to revise his theory. His revised scheme, he maintains, is very similar to its predecessor. Indeed, it is, if anything, more comprehensive. For his original scheme had no place for a rare and enigmatical type of star called white dwarfs. The Dutch astronomer Voûte called attention to one of these in 1919; the faint companion-star of Sirius is another. These are stars of low absolute magnitude and presumably small mass and great density. Now, on Russell's original theory, only the most massive stars can reach the B-type. According to Russell's new theory, these white dwarfs find a place at the very end of the evolutionary sequence. As the life of the average star proceeds, ' there is a process which leads to the actual annihilation of the main mass of the stellar material, with a correspondingly great liberation of energy. The central temperature will then remain nearly constant, and the star steadily decrease in mass, " burning itself away " at the hot centre, gradually growing more dense and opaque and passing down the main sequence.' There remains within the star, however, a certain residue of refractory material which cannot be burnt away even at a central temperature of thirty million degrees. The result of this is contraction and a further increase of surface-temperature, which eventuates in what is called an early spectral type.

At the same time, Professor Eddington has pointed out that though a star may be losing mass by radiation, it may be gaining mass by picking up diffuse matter in space. Thus a star might evolve in one direction and another star in an opposite direction; some stars might be gaining and others losing mass. This idea of alternative sequences formed the basis of a theory outlined in 1918 by Perrine, which did not at the time of its promulgation receive the attention its intrinsic merit seemed to warrant. According to Perrine, the galactic regions are filled with cosmical matter which is swept up by the stars as they move; the energy from the matter swept up being in excess of the energy lost by radiation. The direction of spectral change under such conditions is toward the nebulae. In the regions far removed from the plane, however, where cosmical matter is relatively scarce,

‘ the direction of change will be toward the late types ’. Thus Perrine abandons the effort to fit all the stars into one order of evolution, and declares for two sequences, dependent on location in space.

Dr. J. H. Jeans, the distinguished English mathematician, has for several years devoted his attention to the theoretical side of cosmogony. A pupil of Darwin at Cambridge, he investigated by turn the various tracks of evolution for different bodies in much the same manner as Darwin considered the evolution of the Earth-Moon system. His theoretical studies led to the view that close binary stars were formed by the fission of a single rotating gas. According to Jeans, a giant star cannot divide until near the summit of its temperature curve with a density of about a quarter that of water. Strong support is lent to this view by the observational fact that close binaries are almost invariably of the spectral types B and A. Still more recently, Jeans suggested that Cepheid variation—now generally conceded to be due to the pulsation of gaseous stars—is a symptom of a star’s approaching disruption. But binary stars, as is well known, fall into two distinct classes—close spectroscopic and wide telescopic parts. According to Jeans, the mode of formation has been different in the two classes of binaries. The components of wide binaries originated as adjacent nuclei in the primitive nebulae.

The spiral nebulae have been regarded by Dr. Jeans as true nebulae, and the actual progenitors of normal stars. The stars, he has maintained, are forming under the eye of the observer and are visible to us as knots in the spiral arms. But these speculations are based on the supposedly nebular nature of the spirals, and grave doubts have always been entertained on this point, and doubts are now graver than ever as the result of Dr. Hubble’s success in partially resolving two spirals into stars. Less dependent on observational evidence is Dr. Jeans’ theory of the origin of the Solar System. Like Messrs. Chamberlin and Moulton, Jeans concludes that the Solar System had its origin in the far-away past in the approach of a primitive star to the primitive Sun.

> 'We conjecture that something like 300 million years ago our Sun experienced an encounter of this kind, a larger star passing within a distance of about the Sun's diameter from its surface. The effect of this would be the ejection of a stream of gas towards the passing star. At this epoch the Sun is supposed to have been dark and cold, its density being so low that its radius was perhaps comparable with the present radius of Neptune's orbit. The ejected stream of matter, becoming still colder by radiation, may have condensed into liquid near its ends, and perhaps also partially near its middle. Such a jet of matter would be longitudinally unstable and would condense into detached nuclei which would ultimately form planets. The more liquid planets at the end of the chain would be those of smallest mass; the gaseous centre would form the larger planets Jupiter and Saturn. Owing to the orbital velocity which had been communicated to the planets by the attraction of the passing star, they would not fall back into the Sun, but would describe elliptic orbits, passing fairly near to the Sun's surface at their closest approach. As they passed relatively near to the Sun, the same process as resulted in the formation of planets out of the Sun may have resulted in the formation of satellites out of the planets.'

The theory as originally propounded had one startling corollary. Dr. Jeans considered the length of life of the average star to be of the order of about 300 million years; that is to say, the unit of time in star-life was reckoned at a million years. If such were so, near approaches of the kind assumed in his theory would be of rare occurrence. Accordingly, he gave it as his opinion that 'it is just within the bounds of possibility, although quite, I think, outside the bounds of probability, that our system is unique'. From this assumption of the rarity of planetary systems, the deduction has been made that perhaps, after all, the Earth is the only inhabited world in the Universe, and that our forefathers were not so far wrong in their anthropocentric attitude.

Eddington remarked some years ago that possibly the time-scale adopted by Jeans was too small. It has been left to Jeans himself, however, to modify his own hypothesis, and this he did in November 1924. Eddington had shown that a star's luminosity is a function of its mass, and that as luminosity decreases, mass in all likelihood decreases also. The Einstein theory of relativity, as it happens, gives the rate of exchange between mass and energy, and from this formula Jeans deduced that the Sun is losing mass at the rate of four million tons per second ; but the Sun's mass is so great that this enormous loss of mass is practically negligible. Jeans finds accordingly that the life of the stars is to be measured not by millions but by billions of years. The extended time-scale 'lessens very much the inherent improbability against solar systems being formed by tidal cataclysms ; systems such as our own ought, with the longer time-scale, to be quite common in the sky '. Further, billions of years ago the stars must have been much more closely packed together than now, and this would result in many near approaches and resultant cataclysms giving rise to planetary systems.

'The cosmogonist', Dr. Jeans remarked some years ago, 'can never be justified in stating the results of his investigations with any confidence ; if he must make a positive assertion, the only one he is entitled to make is that in cosmogony we know nothing at all for certain.' While this is eminently true, the astronomer gropes his way amid the problems presented by the solar and stellar systems, seeking for the hypothesis which will best fit the observed facts, and up to the present the tidal theory as now extended and modified would appear to be the most probable yet advanced. The most formidable objection to the theory in its original form was that it seemed to imply the virtual uniqueness of the Solar System, and for this reason alone astronomers were entitled to suspend judgement. For we may confidently conclude that we are no more unique in origin than in location, and that the stellar dwarf which we call the Sun is not the only star which is attended by at least one inhabited world.

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